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METEORS AND FALLING STARS.

By RICHARD A. PROCTOR.

A DISCOVERY has been recently made respecting meteors, by Mr. Denning, of Bristol, which throws quite a new light on the subject of meteoric astronomy. The interpretation of the discovery is indeed marvellous,—and but one interpretation is possible. Thus, when four or five years ago Mr. Denning announced the discovery as already to all intents and purposes made, I felt that the probabilities against what the discovery seemed to prove were so great that even the apparently overwhelming evidence collected by Mr. Denning did not suffice to establish the discovery as real. It seemed to me then, and even now something of the same feeling remains—against my own judgment—that the startling nature of the deduction from the evidence, was such as to make the evidence itself seem untrustworthy. Yet was there much to tempt *me* at any rate to accept Mr. Denning's evidence and the conclusion deducible from it. Despite our consciousness that science requires us to consider the truth only, not what we have thought and therefore wish to be true, we cannot, even in the most earnest search after new truth, but feel some interest in finding conclusions right which we have carefully reasoned out. Now if Mr. Denning's discovery were real, I saw that a theory respecting the origin of meteors which I had advanced several years before, would be supported by such strong evidence that it would be rendered much more obviously probable than it had thus far appeared to be. Possibly this had something to do with my unwillingness to accept Mr. Denning's discovery as real. I have learned to look with suspicion on all evidence which I *wish* to find true. Just as I examined with perhaps unnecessary caution Mr. Brett's fancied recognition of the solar corona when the sun was not eclipsed, because I was then specially interested in showing that the corona is not a mere phenomenon of eclipses, so I looked with doubt on Mr. Denning's startling discovery of meteor systems with permanent radiants because if that discovery were real the theory I had maintained respecting the origin of meteors would be the only theory by which the discovery could possibly (so far as I can see) be explained.

I propose now to discuss afresh what had been already learned respecting meteors and falling stars before Mr. Denning made the discovery which he announced several years ago, and may now be said to have established. This is a necessary preliminary to the explanation of the true meaning of his discovery. I may remark in passing that on this last point as I explained years ago, and particularly noticed in communications addressed to Mr. Denning himself, there can be no manner of doubt or question. Admitting his discovery, as I now do (because he has established it by sufficient observations), the interpretation though utterly amazing is unmistakable. Mr. Denning seems to think this is not so, that accepted theories have to be abandoned, and that, even when that has been done, his discovery remains unexplained and probably inexplicable. But as a matter of fact the difficulty with his discovery is that it is *so obviously explicable in only one way*. Other theories have not to be given up. What has been already proved respecting the orbits of meteors remains still established: it is set on too firm a basis to be shaken by any discovery whatsoever. But the meteors to which Mr. Denning's discovery relates, form a class respecting which entirely new ideas must be adopted. Their existence is no more inconsistent with the doctrine before held (and really demonstrated) respecting the November meteors, the August meteors, and other such systems, than the existence of comets is inconsistent with the doctrine that planets travel in nearly circular paths around the sun. But precisely as the theory of circular orbits will not serve us in dealing with comets, so the theories which have been demonstrated respecting meteor systems travelling in closed orbits utterly fail us in dealing with the meteors which seem to pour in upon the solar system for months (aye and for years) in succession, from the same regions of the star depths.

Let me remark at the outset that some of the mathematical relations involved in the discussion of my subject, though by no means difficult, are not suited to such an article as the present is intended to be. Here, therefore, I shall content myself by using, as wanted, the results which mathematical inquiry indicates. Elsewhere in these pages I will present so much of the mathematical inquiry as may be necessary for the elucidation of my subject.

We start then with the theory established, as I have already mentioned here, by Olmstead of America,—that shooting-stars and meteors are bodies which enter our earth's atmosphere from outside, and had been travelling on parallel paths as they thus entered the air, those parallel paths being parts of their orbits around the sun. Olmstead could prove nothing about the real nature of the paths along which meteors travel, except that the sun must be the ruling body whose attractions the meteors obey, and that although the meteors travel in flights, each individual meteor may be regarded as a tiny planet until its career is brought to a close in our own air. It has always seemed to me to afford fine proof of the quality of Humboldt's mind, that though not himself an astronomer, he recognised the fulness of the evidence in favour of this astronomical interpretation of meteors and falling stars, at a time when mathematical and professional astronomers for the most part pooh-poohed Olmstead's theory as scarcely worth thinking about.

There could be no doubt, even in Olmstead's time, that the theory which he had advanced respecting meteors was sound. First the occurrence of displays of meteors on particular days in the year proved that these bodies must travel around the sun in systems, each system crossing our

earth's track in one place and no more.* Secondly the existence of a radiant point for each system of meteors, this radiant point being fixed among the stars, proved conclusively not only that the paths of the meteors must be parallel, but that the velocities with which the meteors enter our atmosphere must enormously exceed the velocities with which different parts of the earth's surface are carried round owing to her rotation on her axis, and also such velocities as the earth's attraction can impart to matter approaching her from outside.

It will be well to examine both these last-named points somewhat attentively, as they involve considerations bearing importantly on the discovery which has just been made by Mr. Denning.

Now, first, as to the velocities with which different parts of the earth's surface are carried round by her rotational movement. These of course are very small compared with the velocities of meteors travelling from interplanetary space across the earth's orbit. The earth herself travels on her orbit at the rate of 18½ miles per second. Meteors having their aphelia near the orbit of the earth might travel much more slowly than this at the earth's distance. But of such meteors we need take little account; for far the greater number of meteor systems travel on orbits having a greater mean distance than the earth's, and therefore cross the earth's orbit with greater velocity than that with which the earth herself travels. With an orbit not extending indefinitely beyond the solar system, a flight of meteors would travel at the rate of not more than 26 miles per second when at the earth's distance. Taking the average actual velocities of interplanetary meteors at certainly not less than 20 miles per second where they cross the earth's orbit, it is manifest that even the rotational velocity of the earth at the equator could but slightly affect the apparent direction of meteoric motion. For this velocity is but about 17⅓ miles per minute, and the assumed mean velocity of meteors, 1,200 miles per minute, is nearly 70 times as great. In latitute 45 north or south the rotational velocity is barely one-hundredth of the average velocity of meteors. Of course an effect must be produced by this velocity in the case of a meteor-shower lasting several hours, Still it must be very slight.

At first sight it might seem as though the effect of the earth's attraction in deflecting meteors from their course, must be much greater, since the earth can communicate to a body approaching her under her sole influence from without a velocity of nearly seven miles per second. In reality, however, this cause, like the preceding, can produce but slight effects,—as I shall next proceed to explain.

(*To be continued.*)

The Washington monument is reported by the *Scientific American* to be in danger of falling. The area of the foundation, exclusive of a space in the centre not directly loaded, is given as 8,277 square feet, but even with this area the load is said to be 11 tons per square foot without wind.

Platinum has been discovered in New South Wales in connection with gold in the Ophir district. It has been found in the form of small grains in the Hunter and Macleay districts, and a nugget weighing 268 grains was obtained from Wiseman's Creek with alluvial gold. The sand of the sea-coast near Richmond river is also found to contain this precious metal.—*Athenæum.*

* In Guillemin's "Le Ciel," and its English version "The Heavens" (until in revising the work I cut out the passage), the rather absurd mistake was made of presenting the August and November meteor-systems as one system crossing our earth's track in two places! A very slight familiarity with even the most elementary principles of orbital motion should have shown this to be impossible.

PLEASANT HOURS WITH THE MICROSCOPE.

By Henry J. Slack, F.G.S., F.R.M.S.

MOLECULAR MOTION.

EVERY microscopist should be acquainted with the curious facts commonly spoken of in books as "molecular motion," or, by the French, as the "Brownian movements," because the great botanist, Robert Brown, though not its discoverer, first succeeded in calling general attention to it as a phenomenon common in plants, and likewise in water containing minute particles. The writer remembers once possessing an old pamphlet sold by a London optician, and giving an account of various experiments with different bodies, including metals, which when sufficiently reduced in size and suspended in water, were seen under the microscope to be in a perpetual fidgetty motion. Those insoluble substances which did not differ much in specific gravity from water were found the easiest with which to display this appearance, and it is very readily seen by just touching a waterdrop on a glass slide with a bit of gamboge, covering with thin glass, and subjecting it to a quarter-inch or higher power.

Before describing further experiments, it may be as well to point out the objection to the common terms "Molecular" and "Brownian" movements. Formerly, the word molecule meant vaguely any very small mass; but both physicists and chemists now limit its meaning. The physicist regards it as the unit—the smallest particle of any substance that exhibits force and motion. The chemist means by it the smallest particle of any substance capable of manifesting the properties belonging to that substance. His molecule of sugar is the smallest possible bit of sugar; but he knows that sugar is a compound of hydrogen, carbon, and oxygen, and that its molecule contains many atoms of those substances. If any of the bodies that figure as "simple" in chemical books are really so, then their atom and molecule would be the same; but if oxygen, for example, should ever be decomposed into two or more distinct substances, the chemists' molecule of that gas would be an aggregate of the molecules of its components. The smallest particle of any substance to which the term molecule is now applied is inconceivably smaller than any object the microscope can show, and the least visible particle contains millions upon millions of ultimate molecules, far exceeding the power of intellect or imagination to follow.

Our experiments, then, relate to the movements of microscopic particles in liquids, and it may be broadly affirmed that sufficiently minute particles of most bodies not quickly soluble in water exhibit what look like spontaneous movements when immersed in that fluid. The best account of the matter known to the writer is contained in a paper of Professor Stanley Jevons in the *Quarterly Journal of Science* for April, 1878. He traced the subject back to Stephen Gray, in whose essay, published in the "Philosophical Transactions for 1696," the moving particles were spoken of as "animated," and some he considered to be "insects." John Bywater, cited by Jevons, mentioned them in 1819, and rejected the notion of vitality, because he found that, whereas animalcules were killed by boiling, sandstone particles that had just been made red hot moved as well as those that had not been heated at all. Professor Jevons proposed to call the motions *pedetic*, or in the singular *pedesis*, from the Greek for leaping or bounding, "this," he says, "being the correct description of the phenomenon when seen in perfection."

The reader who will make a few experiments may determine for himself how far the motions are of the leaping sort. Gamboge, for example, as I see it, has its particles in a constant fidget, never for a fraction of a second quiet, but varying its motions in extent. All the particles look round, and it makes little or no difference whether they are some distance apart or close together. The term "leaping" suggests intermittence—a pause and a jump, but it may certainly apply to a sudden extension of range. There are no pauses. Professor Jevons' paper says, "The distance through which a particle moves at one bound is usually less than 1-5,000th part of an inch, and those of a greater diameter than 1-5,000th of an inch are seldom seen to move." Sudden extensions of the spaces traversed by any particle are common enough, and they appear like what anyone may imitate on a large scale by making a little dance on a square foot of carpet, and every now and then moving a yard.

Very fine pumice-stone dust presents a different aspect from gamboge. Many of its mobile particles are elongated, and quiver as they go.

If a penknife is rubbed on a fine hone, moistened with water, and a very little of the product of the friction transferred to a clean water-drop on a glass slide, it will be found, on magnification, that the hone has supplied coarse, angular, rocky particles, and the steel very much smaller ones, which exhibit the movements. A drop of old common ink was one of Professor Jevons' favourite objects. He found its particles "in such rapid motion as to cause a boiling or swarming appearance." This kind of motion is frequently seen when living organisms discharge a multitude of germinal or other particles. Fine, pure China clay Jevons found the most convenient for experimenting with.

Light and darkness made no difference in the results he obtained, and he was unable to confirm the opinion that heat accelerated the motions. "A mixture of charcoal-powder and boiled water he surrounded with ice, and a similar mixture was placed in boiling water, and maintained at 100 C. At the end of an hour the heated mixture had deposited nearly all the charcoal, whereas the ice-cold water had as much in suspension after eight hours." China-clay gave the same results.

If the particles are made small enough in proportion to their specific gravity, to neutralise their tendency to sink, their motion may go on for days, months, and even years, if evaporation is prevented. Substances containing silica are readily susceptible of the motion. Oxides, chalk, fluor-spar, galena, and many others have less tendency to it. Mineral acids, salts, and most soluble substances arrest the motion of china clay, and are generally unfavourable to the action; but gum arabic in weak solution augments it. Pure distilled, or rain, water is much more favourable to it than water holding minerals or chemicals in solution.

Professor Jevons traced a curious connection between the state of the water most favourable to pedesis and that which produced the best results with Armstrong's electric boiler. In the best form of that instrument—of which a fine specimen was at one time shown at the Polytechnic—electricity is generated by the friction of water-particles escaping with high-pressure steam through nozzles of partridge-wood. Details will be found in Noad's "Manual of Electricity," Part I. "Faraday," he says, "found that electricity is never excited by the passage of pure steam, and is only manifested when water is at the same time present." Pure water, as in the case of pedesis, answered best, and "the smallest drop of sulphuric acid, or a little crystal of sulphate of soda, dissolved in water, prevented the evolution of electricity." So remarked Jevons, citing Faraday.

If many experiments are contemplated, a very small agate pestle and mortar should be provided. This is the best for fine grinding, but when very hard substances are operated upon, a little of the agate is ground up with them. Of the substances I have tried, fine particles of antimony have proved the most lively, and though very minute ones are most active, those large enough to sink rather quickly still show the peculiar motion.

If we pass from small experiments under the microscope to a consideration of large operations in nature, we see that pedesis produces very important results. A huge river, for example, like the Mississippi, brings down in the flood season millions of tons of mud a day, and pedesis materially extends the time it takes for the smaller particles to fall and constitute a submarine deposit. If the water of the mighty river were unfavourable to their influence, the deposits would occur more rapidly, and the finest strata would be much coarser than under the existing conditions. The longer the fine particles are suspended, the further they are carried by the stream, and all the coarser have fallen before they reach their final destination. Under a great variety of circumstances the pedetic movements must facilitate chemical and other changes. They are doubtless subservient to digestion, assimilation, and growth, and also to dissolution and decay. They assist in the life processes of the smallest organism, and they regulate the deposition of vast geological formations, such as fine slates, shales, and lithographic stones.

OPTICAL RECREATIONS.

By a Fellow of the Royal Astronomical Society.

(*Continued from p. 167.*)

BEFORE proceeding to deal with the phenomena of colour proper, we may conclude this portion of our subject by some mention of those of Diffraction and Interference, especially as they may be well illustrated by curious and beautiful experiments, made with simple materials within the reach of everybody. Moreover, such experiments possess a high theoretical value; inasmuch as they supply crucial tests of the truth or falsehood of the two hypotheses which have been framed to explain the nature of light itself; and to which reference will be found on p. 352 of our fifth volume. As there stated, Newton held that light consisted of actual material elastic particles emitted by the luminous body; and alleged that, if the now universally accepted theory that light has its origin in the undulations of something ("the æther") which fills all space were true, shadows could not exist; for that the light-waves would bend round the edges of opaque bodies and set the æther in motion behind them; a thing which—as he asserted—never happens. He was right so far in his theory, but wrong as to his facts; for this bending of light, partly, as it were, round a corner, actually does take place, and such bending is known as Diffraction. It is with some of the phenomena to which it gives rise that we have now to deal.

For their study we require either a physical point of light (like a star, or the image of the sun formed by a lens of short focus), or an extremely thin line of light, such as is obtained through a very narrow slit brightly lit by the sun; or, better still, from the image of such a slit in a strong cylindrical lens. We will first employ a point of light, and commence with our most elaborate form of

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experiment. It is illustrated in Fig. 39. Here S is the shutter of a room which we have totally darkened. In this shutter is pierced a small hole, *h*, through which the sun shines. We cover this with a piece of red glass, *r*, *g*. because if we use white, instead of monochromatic light, our shadows will be fringed with the so-called "rainbow" or prismatic colours, and for our present purpose we wish to exclude chromatic phenomena. Very well, then, our narrow beam of red light passes through a condensing lens, L, of short focal length, and is brought to a focus at *f*, from the brilliant point formed at which the rays

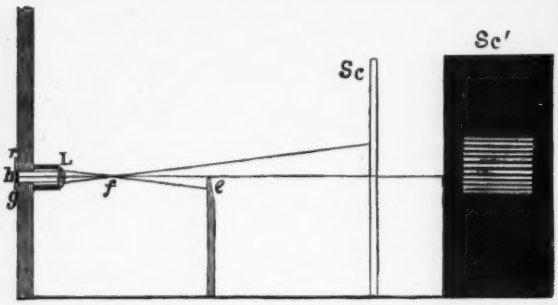


Fig. 39.

diverge as seen in the figure. Beyond the focus, say at *e*, is placed a sharp opaque edge (a new carving-knife with a straight edge answers well) so as to cut off part of the luminous cone, the remaining portion travelling onwards, and being intercepted on the white screen, S c, seen edgeways in the figure, S c′ to the right of it showing a front view of it. Now, it might be expected that within the limits of the sheaf of rays falling on the screen, the whole space would be equally illuminated; but, instead of this, it will be seen to be crossed by a series of light and dark bands parallel to the edge of the carving-knife; most sharply, though not quite sharply, defined as they approach the geometrical shadow, and getting less defined and fainter as they recede from it. If for our red glass we substitute green, the fringes get narrower, and with blue narrower still. As we have, so far, said nothing about the distinction between rays of various degrees of refrangibility, it must suffice simply to mention this fact here. If, instead of our knife-edge, we interpose a very fine wire in the cone of rays diverging from *f*, we shall get fringes outside of the geometrical shadow as before; but we shall also get a series of very narrow light and dark bands within the geometrical shadow itself. A small circular disc, cut out of tin, and interposed in the course of the rays, gives a *bright* spot on the screen, surrounded by concentric rings. With white light these rings are beautifully coloured. Solar and lunar halos belong to this class of phenomena, and are caused by particles of mist diffracting the light. These may be imitated, too, by strewing lycopodium powder on glass and regarding any bright light through it.

But we may produce these beautiful effects without any dark room at all; in fact, if we take two thin black cards, and in each of them cut a *very* narrow slit. Then, by lighting one of these slits by direct sunlight and viewing it through the other, we shall observe the effect shown in Fig. 40. Here, again, though, if we wish to exclude chromatic fringing, we ought to have a piece of red glass at the back of card No. 1, so that the sun passing through it shall give a red line for our examination.

The possessor of a telescope will be able to produce the most curious diffraction effects by the aid of variously shaped diaphragms placed over his object-glass. Fig. 41 exhibits some of these effects, obtained by viewing Regulus through a series of diaphragms placed over a 4-in. object-glass with a power of 160. A shows the result when the light passed through the opening formed by two concentric squares; B, that obtained when the aperture was triangular. C, the effect with a series of five small equilateral triangles

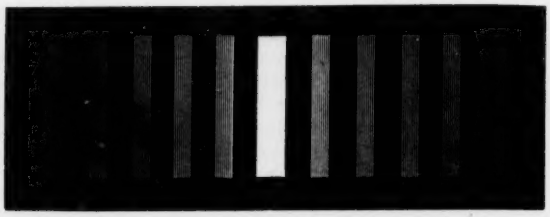


Fig. 40.

with their apices towards the centre of the diaphragm. D, the appearance afforded by a rhomboidal opening; while, finally, E shows what was seen when the object-glass was covered with that perforated card on which ladies work book-markers. The most familiar of all diffraction phenomena to the observer with the telescope—we mean, of course, the rings surrounding a bright star when viewed with a high power—need no illustration here. They will be found depicted on p. 201 of our first volume.

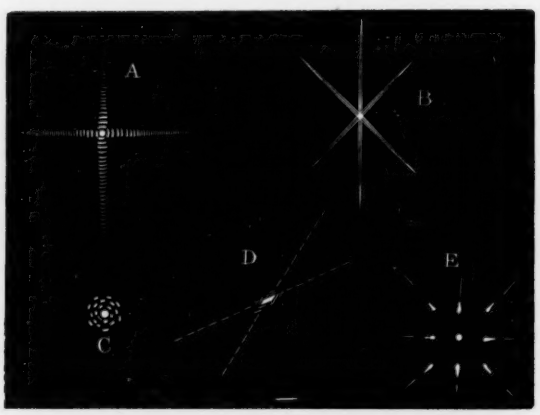


Fig. 41.

Cognate effects to some of those which have been described above may be obtained by placing opposite to *f*, in Fig. 39, two plane metallic mirrors inclined to each other at a *very* small angle, or with their surfaces almost in the same plane. These will reflect the diverging beams, so that they meet at a point—or, more rigidly, a line of light—parallel to the edge where the mirrors are in contact; on either side of which we shall find the bands as before.

Without going into abstruse mathematical demonstration foreign to the purpose of these essays, it may suffice to point out that, while on the emissive hypothesis of light these beautiful appearances are inexplicable, inasmuch as two material particles can never, under any circumstances, annihilate each other, upon the undulatory theory they become intelligible enough. For, as long as the æther is still, we have no sensation of light; but when waves are set up in the æthereal ocean and reach our eyes, then does that sensation at once arise. In other words, if our luminiferous ocean is *smooth*, all is darkness; when undulations or waves agitate it, there is light. But

it may be smooth for either one of two reasons — no waves of any description may agitate it at all, or the crest of one wave may fit into the hollow of another, and so reduce that part of the surface to a dead level. Now, let us suppose two sets of waves set up in an elastic system from two separate points, and let these points be so situated that the vibrations (light) from them reach a third point either together or at times differing by exactly one wave-length, two wave-lengths, &c. Then two wave-crests must simultaneously reach that point, with the result of *doubling* the light there. Suppose, though, that the waves from our two points reach the third at times differing by half a wave-length, a wave-length and a half, &c., then it will be seen that the crest of one wave must fit into the hollow of the other, produce a dead level, and consequent darkness. It is from one wave thus interfering with another that the phenomenon has derived the name of Interference. A very little thought will suffice to show how this happens in the case of waves from the two mirrors; and this idea once grasped, it will not be difficult to see that the sharp edges of the openings, or bodies casting the shadows in our first experiments, by placing obstacles in the way of the rectilinearly propagated light which strikes them, set up fresh systems of waves; those from each edge interfering with each other, and alternately doubling the amount of light and annihilating it.

THE PHILOSOPHY OF CLOTHING.

By W. Mattieu Williams.

VI.—THE PROTECTING POWER OF DIFFERENT MATERIALS.

THE philosophy of the problem enunciated at the end of my last was for the first time systematically studied by Benjamin Thompson, afterwards Count of Rumford. In my previous papers on "The Chemistry of Cookery," I introduced this remarkable man to my readers as a philosophical cook. I have now to describe some of his exploits as a tailor.

Between 1777 and 1784, he served the British Government both in naval and military capacities, having previously distinguished himself rather suddenly as a diplomatist. He rapidly rose to the rank of colonel, but could get no fighting to do, although he went to Jamaica, and also offered his services to Turkey for the purpose. The world had suddenly become peaceful for a short time.

He therefore very cleverly managed to introduce himself to the Elector of Bavaria, obtained a formal invitation to enter his service, and returned to London to ask permission from the King to do so.

This was given, and with it the honour of knighthood. Sir Benjamin then migrated to Munich, set to work at once in first studying the social and military condition of Bavaria, and then devising the reforms that were necessary.

I must not turn aside to enumerate these, however interesting, having at present only to do with one of them —viz., the clothing of the Bavarian army. In reference to this, he says:—"Having in the year 1784, with his Majesty's gracious permission, engaged myself in the service of the Elector Palatine Reigning Duke of Bavaria, I have since been employed by his Electoral Highness in various public services, and particularly in arranging his military affairs, and introducing a new system of order, discipline, and economy in the troops." I may add that, on carefully studying his own account of this "new system," I have become convinced that the present military eminence of the German Empire is due to the fact that the whole of Germany subsequently carried out Thompson's original system of order, discipline, and economy in all its details, even to those of clothing material, as will be presently seen. In this respect, Germany owes as much to Thompson as to Bismarck and Moltke.

His tailoring achievements were primarily connected with his scheme of military education. He tells us that in all his operations he endeavoured "to unite the interest of the soldier with the interest of civil society, and render the military service, even in time of peace, subservient to the *public* good;" and "to establish a respectable standing military force, which should do the least possible harm to the population, morals, manufacture, and agriculture of the country, it was necessary to make soldiers citizens and citizens soldiers."

Industrial training supplementing the primary instruction in his military schools was an important element of this business, one department of industry being the spinning and weaving of the materials, and the making-up of the garments of the soldiers by the soldiers themselves.

What material? was the first question. This scientific and practical reformer had no respect for established usages, however ancient, unless he could discover good reasons for their existence. His question was not what the soldiers *had* worn, but what they *should* wear, what would clothe them the most comfortably and decently at the smallest cost.

The first use of clothing being to keep the wearer warm his first inquiry was directed to find the best material for this purpose. He saw at once that clothing did this by resisting the passage outwards of the animal heat, *i.e.*, by virtue of its resistance or low conductivity, or "non-conducting power," as he called it.

Ordinary tailors would have tried experiments on human beings by supplying different kinds of clothes, and consulting the opinions of wearers concerning their relative merits. But Thompson was too well acquainted with the disturbing influence of prejudice to do this, and therefore constructed a model soldier that would tell the truth, the whole truth, and nothing but the truth.

This model wearer of clothing was a thermometer, the bulb of which represented the body of the soldier. By simply clothing it with determinate quantity of various material, raising it to a given temperature, and then surrounding it with definite cooling agencies, he was able to measure the relative powers of the different materials in resisting the passage of heat.

He used for this purpose a mercurial thermometer, whose bulb was about 55-100th of an inch in diameter, and its tube about ten inches long. This was suspended in the axis of a cylindrical glass tube about three-quarters of an inch in diameter, ending with a globe 1 6-10th inch in diameter, in such a manner that the centre of the thermometer bulb occupied the centre of the globe, thus leaving a surrounding space to be occupied by the material to be examined. The thermometer tube was graduated with 800 divisions between the freezing and boiling-points of water —*i.e.*, a Reaumur scale divided to tenths of degrees. The thermometer was held in its place by a long cork stopper.

He named this a "passage thermometer," and constructed it with many modifications to suit various experiments, but the principle was the same in all. I should add that he communicated several papers to our Royal Society describing many other experiments with such thermometers, their object being to determine the conducting power of mercury, water, various vapours, and vacuum, besides those on clothing materials.

He describes his method of clothing the bulb as follows:

"The thermometer being taken out of the cylindrical tube, about two-thirds of the substance which is to be the subject of the experiment are introduced into the globe; after which the bulb of the thermometer is introduced a few inches into the cylinder; and, after it, the remainder of the substance, being placed round about the tube of the thermometer; and lastly, the thermometer being introduced farther into the tube, and being brought into its proper place, that part of the substance which, being introduced last, remains in the cylindrical tube above the bulb of the thermometer, is pushed down into the globe, and placed equally round the bulb of the thermometer by means of a brass wire, which is passed through holes made for that purpose in the stopple closing the end of the cylindrical tube."

The temperature he selected as the starting-point was 70° Reaumur=190° Fahr. He preferred this to the boiling-point, as he could obtain it accurately by first plunging the whole apparatus duly charged into nearly boiling water, then allowing it to fall to 70°, and immediately plunging it into a mixture of pounded ice and water, where, by the aid of a little agitation, it remained steadily at the freezing-point.

The materials he first examined were raw silk, as spun by the worm; sheeps' wool, cotton wool, linen in the form of the finest lint, being scrapings of very fine Irish linen, the finest part of the fur of the beaver, separated from the skin and from the long hair, the finest part of the fur of a white Russian hare, and eider-down. Sixteen grains of each of these were used by placing them in the globe of the passage thermometer in the manner above described, arranging them equally around the mercurial bulb.

As the interstices of these bodies were filled with air, his first experiments were made with air alone, with the following results:—

Heat lost.	Expt. No. 1. Time elapsed.	Expt. No. 2. Time elapsed.	Heat acquired.	Expt. No. 3. Time elapsed.
70°	—	—	10°	—
60°	38″	38″	20°	39″
50°	46	46	30°	43
40°	59	59	40°	53
30°	80	79	50°	67
20°	122	122	60°	96
10°	231	230	70°	175
Total times.	576	574	—	473

The figures in the second and third column tell the number of seconds occupied in losing the successive amounts of heats between 70° and 10° Reaumur. The third experiment shows the time required for gaining the temperatures stated in the fourth column, when the passage thermometer was immersed in boiling water at the moment it registered 10°. (The Reaumur freezing-point is 0.)

The following table shows the results with the various substances therein mentioned, the quantity in each case (excepting the first, which is a repetition of the first of the above-named experiments) being 16 grains.

Heat lost.	Air.	Raw silk.	Sheep's wool.	Cotton wool.	Fine lint.	Beavr's fur.	Hare's fur.	Eider-down.
	Exp. 1	Exp. 4	Exp. 5	Exp. 6	Exp. 7	Exp. 8	Exp. 9	Exp.10
70°	...	...	...	...	...	...	...	...
60°	38″	94″	79″	83″	80″	99″	97″	98″
50°	46	110	95	95	93	116	117	116
40°	59	133	118	117	115	153	144	146
30°	80	185	162	152	150	185	193	192
20°	122	273	238	221	218	265	270	268
10°	231	489	426	378	376	478	494	485
Total times	576	1284	1118	1046	1032	1296	1315	1305

The clothing value or "warmth" of these substances *as thus applied* varies directly with these figures representing their power of resisting the passage of heat, or their "non-conducting power."

He says: "I acknowledge that the differences of warmth of these substances were much less than I expected to have found them," and adds that this might arise from their volumes or solid contents being different, though their weights were the same. He therefore made further experiments, which I will describe in my next.

IS THE DIAMETER OF THE PUPIL OF THE EYE AN EQUIVALENT OF THE LIGHT'S INTENSITY?

By John Gorham, M.R.C.S. Eng.

(From a paper read before the Royal Society in November, 1884.)

(*Continued from page* 300.)

TO obtain additional and still more conclusive evidence of the correspondence between the pupil's size and the light's intensity, experiments were now made on a larger scale, the pupil of the eye being measured every consecutive hour, from 9 a.m. to 7 or 8 p.m., daily, for a period of three months. From these experiments it was clearly and satisfactorily developed, that certain definite magnitudes of the pupil repeated themselves at definite periods or intervals, either of hours, days, or months, and that these magnitudes coincided with the kind of light by which they had been stimulated into existence.

Hours of the day.	Mean diam. of the pupil in hundredths of an inch (from 1,000 experiments) in:— April.	May.	June.
9	110	94	88
10	105	88	80
11	100	83	78
12	109	79	78
1	103	83	75
2	107	86	80
3	110	87	80
4	107	90	83
5	115	94	84
6	122	102	89
7	154	111	98
8	187	140	125

From this Table, which embodies the results of one thousand experiments, it appears that during the three consecutive months of April, May, and June, 1884, the pupil's magnitude became gradually smaller and smaller, thus keeping pace with the gradual increase in the light's intensity. Again, that if we take the mean of all the diameters for each hour, and set them in parallel columns, we find that on any given hour the pupil is more dilated in April than in May, and in May than in June, thus coinciding with the light's increasing brilliancy at these successive periods.

Again, it was reasonable to anticipate that the mean light would be stronger, and, if so, that the mean size of the pupil would be smaller on each succeeding month. It was found accordingly that the mean size of the pupil in April was = ·119 in.— in May ·094, and in June ·086 in.

Abundant evidence has been adduced to show that the pupil's movements and the light's intensities are mutually reciprocal. In no single instance has the pupil been found to make, so to speak, a false move. It has never contracted when stimulated by a weak light, or dilated by a strong

one. All its movements have been found to harmonise with our anticipations. Its fluctuations are synonymous with the light's fluctuations, and it is possible to control the size of the one by selecting the quality of the other. In short, had not the pupil been submitted to these periodical tests it might still have remained to us a mere passive organ, incessantly on the shift in obedience to the light which controls it, but altogether useless as a measure of that light.

Absolute Intensities.

Hitherto we have restricted our observations to lights of relative intensities, by which we have been enabled to compare the intensity of two or more sources of light without knowing the absolute illumination of either of them. It remains to inquire whether, in response to a light of absolute brilliancy, the pupil is enabled to indicate in definite magnitudes the quality of the light, varying as it does with the square of the distance from the luminous body in obedience to the general law of radiant forces. Whether, for instance, at, say, twice the distance from any luminary, the pupil will so dilate to a given magnitude which shall exactly represent in a given fraction the weaker light known to be of one-fourth the intensity.

The most correct method of investigating the photometric powers of the pupil with which I am acquainted is obtained by the aid of a candle-flame, the absolute intensity of which, whatever that may be, is always the same; which may be used in units, or multiples of those units (area), and which may be removed at pleasure to any distance from the eye. These properties are combined in a Sugg's standard candle, which, consuming as it does two grains of its own material by weight per minute, emits an agreeable light.

To obtain the proper effect we place a white surface perpendicularly behind the candle one foot from the eye. We can now easily find the two discs seen through the photometer by looking through the flame upon the white screen, *both eyes being open*. Measured in this way my own pupil becomes dilated to the 0·15 of an inch, and never varies. On now removing the candle to a distance of two feet the eye will receive a light equal to a fourth of the intensity ($2^2=4$). Hence if a single candle illuminates a body to a certain extent at the distance of a foot, it would require four candles at the distance of two feet to produce equal illumination. Accordingly, on placing four candles at two feet my pupil remains steadily dilated to the ·15 inch as before.

This experiment affords strong corroborative evidence of the appreciative powers of the pupil in exactly measuring the intensity of *four* lights, which, at a certain definite distance specified by a well-known law, should be exactly equivalent to the intensity of *one* light only at another distance according to the same law.

The absolute brilliancy (B) of a luminous body is proportionate to the absolute intensity (I) of the luminary, multiplied by the luminous points of its illuminating surface (*e.g.*, number of candles) (S), divided by the square of the distance (D^2) of the illuminated object from it. The brilliancy of the illumination will then be expressed by:—

$$B = \frac{I \times S}{D^2}$$

When by the formula the absolute brilliancy of a candle at the distance of one foot from the eye will be equal to:—

$$B = \frac{\cdot 15 \times 1}{12} = \cdot 15 \text{ in pupil measure.}$$

And of four candles at two feet from the eye will be equal to:—

$$B = \frac{\cdot 15 \times 4}{2^2} = \cdot 15$$

Where the equality in the two brilliancies obtained from different sources at different distances shows the remarkably appreciative power of the pupil.

It will be observed that the process of equalising the two illuminations becomes at the same time the measure of the number of candles used in the experiment. Hence, by an extension of the same principle, the candle-power of any luminous body may be found; thus, if one candle at one foot is equal in intensity to four candles at two feet, it will be similarly equal to nine candles at three feet where the number of candles in each case is found by taking the square of the distance. Let it be required, for instance, to find the candle-power of a gas-flame. Suppose, now, that the distance required to equalise the illuminations (that is to make both appear equal to the size of the pupil at one foot from a candle) is equal to six feet, then the square of six being equal to thirty-six, the brilliancy of the gas-flame will be equal to that of thirty-six candles.

OTHER WORLDS THAN OURS.

A WEEK'S CONVERSATION ON THE PLURALITY OF WORLDS.

By Mons. de Fontenelle.

With notes by Richard A. Proctor.

THE FIFTH EVENING.

(*Continued from p. 283.*)

"YOU have no reason," said I, "to pity the inhabitants of a comet; yet I suppose you will think their condition lamentable, who inhabit a vortex, whose sun comes in time to be quite extinguished, and consequently live in eternal night."

"How!" cried the Marchioness, "can suns be extinguished?"

"Yes, without doubt," said I; "for people some thousand years ago saw fixed stars in the sky which are now no more to be seen; these were suns which have lost their light, and certainly there must be a strange desolation in their vortexes, and a general mortality over all the planets: for what can people do without a sun?"

"This is a dismal fancy," said the lady. "I would not, if I could help it, let it come into my head."

"I will tell you, if you please," I reply'd, "what in this particular is the opinion of learned astronomers. They think that the fixed stars which have disappear'd are not quite extinguish'd, but that they are half suns—that is, they have one half dark, and the other half light; and turning round upon their own axis or center, they sometimes show us their light side, and afterwards turning to us their dark side, we see them no more. To oblige you, madam, I will be of this opinion, because it is not so harsh as the other; but I cannot make it good, but in relation to some certain stars; because, as Huygens has lately observ'd, those stars have their regulated times of appearing and disappearing, otherwise there could be no such thing as half suns. But what shall we say of stars which totally disappear, and never show themselves again after they have finish'd their course of turning round upon their own axis? You are too just, madam, to oblige me to believe, that such stars are half suns. However, I will try once more what I can do in favour of your opinion: The suns are not extinct,

they are only sunk so low into the immense depth of heaven, that we cannot possibly see them: in this case the vortex follows his sun, and all's well again. 'Tis true, that the greatest part of the fixed stars have not this motion, by which they remove themselves so far from us, because at other times they might return again nearer to us, and we should see them sometimes greater, and sometimes less, which never happens. But we will suppose that none but the little, light, and most active vortexes, which slip between the others, make certain voyages, after which they return again, while the main body of vortexes remain unmov'd. 'Tis likewise very strange that some fixed stars show themselves to us, spending a great deal of time in appearing and disappearing, and at last totally and entirely disappear. Half suns would appear again at their set and regulated time. But suns which should be sunk low into the depths of heaven would disappear but once, and not appear again for a vast space of time. Now, madam, boldly declare your opinion: Must not these stars of necessity be suns, which are so much darkened as not to be visible to us, and yet afterwards shine again, and at last are wholly extinct?

"How can a sun," said the Marchioness, "be darkened, and quite extinguish'd, when it is in its own nature a fountain of light?"

"It may be done, madam," said I, "with all the ease in the world, if Descartes's opinion be true,* that our sun has spots: now, whether these spots be scum or thick mists, or what you please, they may thicken and unite, till at last they thicken the sun with a crust, which daily grows thicker, and then farewell sun. We have hitherto 'scap'd pretty well; but 'tis said, that the sun for some whole years together has look'd very pale: for example, the year after Cæsar's death, it was this crust that then began to grow; but the force of the sun broke and dissipated it: had it continued, we had been all lost people."

"You make me tremble," reply'd the lady. "And now I know the fatal consequences of the sun's paleness, I believe, instead of going every morning to my glass, to see how I look, I shall cast my eyes up to heaven, to see whether or no the sun looks pale."

"Oh, madam," said I, "there is a great deal of time required to ruin a world."

"I grant it," said she; "yet 'tis but time that is requir'd."

"I confess it," said I; "all this immense mass of matter, that composes the universe, is in perpetual motion, no part of it excepted: and since every part is moved, you may be sure that changes must happen sooner or later; but still in times proportion'd to the effect. The antients were pleasant gentlemen, to imagine that the celestial bodies were in their own nature unchangeable, because they observed no change in them; but they did not live long enough to confirm their opinion by their own experience; they were boys in comparison of us. Give me leave, madam, to explain myself by an allegory: If roses, which last but a day, could write histories, and leave memoirs one to another; and if the first rose should draw an exact picture of his gardener, and after fifteen thousand rose-ages it should be left to other roses, and so on still to those that should succeed without any change in it; should the roses hereupon say, we have every day seen the same gardener, and in the memory of roses none ever saw any gardener but this, he is still the same he was; and therefore certainly he will not die as we do, for there is no change at all in him. Would not these roses, madam, talk very foolishly? And yet there would be more reason in their discourse than there was in what the ancients said concerning celestial bodies; and tho' even to this very day there should appear no visible change in the heavens, and the matter of which they are made should have all the signs of an eternal duration, without any change; yet I would not believe them unchangeable till I had the experience of many more ages. Ought we, whose lives are but a span, to make our continuance the mensuration of any other being? 'Tis not so easy a matter to be eternal. To have lasted many ages of men, one after another, is no sign of immortality."

"Truly," says the Marchioness, "I find the worlds are far from being able to pretend to it; I will not do them so much honour as to compare them to the gardener who lived so much longer than the roses. I begin to think them like the roses themselves, which blow one day, and die the next; for now I understand that if old stars disappear, new ones will come in their room, because every species must preserve itself."

"No species, madam," said I, "can totally perish; some perhaps will tell you that such new stars are suns, which return to our sight again after they have been a long time hid from us in the profundity of heaven; others may tell you they are suns cleared from that thick crust which once covered them. If I should think all this possible, yet I likewise believe that the universe may be fram'd in such a manner that from time to time it may produce new suns. Why may not that matter, which is proper to make a sun, be dispers'd here and there, and gather itself again at long run into one certain place, and lay the foundation of a new world? I am very much inclin'd to believe such new productions, because they suit with that glorious and admirable idea which I have of the works of Nature. Can we think that all-wise Nature knows no more than the secret of making herbs and plants live and die by a continual revolution? I am verily persuaded (and are not you so too, madam?) that Nature, without much cost or pains, can put the same secret in practice upon the worlds."

"I now find," says she, "the worlds, the heavens, and celestial bodies so subject to change, that I am come to myself again."

"To recover ourselves the better," I reply'd, "let us say no more of these matters. We are arriv'd at the very roof and top of all the heavens; and to tell you whether there be any stars beyond it, you must have an abler astronomer than I am. You may place worlds there, or no worlds, as you please. 'Tis the philosopher's empire to describe those vast invisible countries which are and are not, or are such as he pleases to make 'em. It is enough for me to have carried your mind as far as you can see with your eyes."

"Well," says the Marchioness, "I have now in my head the whole system of the Universe. How learned am I become!"

"Indeed, madam," said I, "you are pretty knowing; and you are so with the advantage of believing or not believing anything I have said. For all my pains I only beg this favour, that whenever you look on the sun, the heavens, or the stars, you will think of me."

* There is something essentially French about this calm description of a discovery of Galileo's as an "opinion" of Descartes.—R. P.

An important work on the "Chittagong Hill Tribes," by Dr. E. Riebeck, translated by Professor Keane, is announced by Messrs. Asher & Co. To the ethnologist, the anthropologist, the zoologist, and the meteorologist this sumptuous volume can hardly fail to prove of high interest.

CHAPTERS ON MODERN DOMESTIC ECONOMY.

XXIII.—THE FRAMEWORK OF THE DWELLING-HOUSE

STRUCTURAL EXAMPLES (*continued*).

THE DISPOSAL OF HOUSEHOLD REFUSE.

IN the valuable contributions to popular hygiene, edited by Dr. Shirley Foster Murphy,* the reasons for the institution of a "manhole" on the course of the household drain are given as follows:—"It thus gradually came to be recognised that for the proper ventilation of drains it was necessary to have an inlet or inlets for air, as well as an outlet or outlets, and it soon came to be seen that the lower the level at which the inlet was placed, the more likely was it to act continually as an inlet for air. So openings were boldly made into the drain itself, on the house side of the trap, from the level of the ground or thereabouts, and it was found to the astonishment of many that, provided there were no places in connection with the drain where actual lodgment of foul matters could take place, such openings prove no nuisance whatever. This point having been reached, a variety of 'disconnecting traps' were devised, in all of which there is an inlet for fresh air to the drain on the house side of the trap, and in one of which—the Edinburgh air-chamber disconnecting-trap—there is a long open channel interposed between the U-trap and the house-drain. Most of these traps, however, while affording an inlet for fresh air, do not afford a sufficient means for getting at the drain and at the U-trap at any time. Neither, with the exception of the one just mentioned, is the inlet for air as large as is desirable.

"Hence the practice, which has been described in preceding pages, of building a manhole or air-chamber in brickwork on the course of the drain, carrying the drain through the bottom of it, as first suggested by Mr. Rogers Field, by means of channel-pipes, and fixing the syphon or U-trap at the lower end of these. The manhole may be covered by an iron locking grating, or, where this is considered undesirable, by an iron locking door, air-inlets being provided by channels carried into the manhole from suitable positions. One great advantage of this plan, besides the facility it affords for getting at the drain and the trap, is, that if at any time air escapes from the air-inlets, the air that comes out is the last fresh air that has entered the manhole, and so, practically, these inlets never become a nuisance."

In "Our Inventors' Column" last week a description was given of Mr. G. C. Davies's "Cast-iron Inspection or Access Pipe for House-drains." In so far as inspection goes, the form of the opening, its cover, and the material out of which it is made, all contribute to make it one of the most secure contrivances that have ever come before our notice, but it can never be substituted for a manhole, since no provision is made for ventilation, one of the most, if not the most, important item in drain construction.

Where sewage discharges into water affected by tides, as into a tidal river or into the sea, the backflow is often sufficient to force the foul matters into the domestic drains, and thus to give rise to evils of the very worst description, more especially in cases where the tide rises high above the sewer level. In order to prevent this undesirable regurgitation, an ingenious appliance has been invented by Mr. F. Dyer, of 66, High-street, Camden-town, N.W., which is perfectly effectual. Dyer's "Patent Automatic Tidal Valve Trap," shown at Fig. 22, consists of a body preferably made of cast-iron of any size to suit the requirements of the drain-pipe to which it is to be fixed; the inlet or house end of the valve trap is higher than the outgo. The valve is a copper ball of unusual thickness, which is made to press against specially-prepared india-rubber and gun-metal seatings when it is called into play. Its action is described as follows:—The tide or flood, when backing up the drain, enters the outgo branch, rises in the chamber, and floats the ball, which, as the water rises, approaches the inlet or house-end branch, and finally beds itself firmly upon the seating, thus effectually closing the orifice, and preventing the water from entering the building. Consequent upon this action, the greater

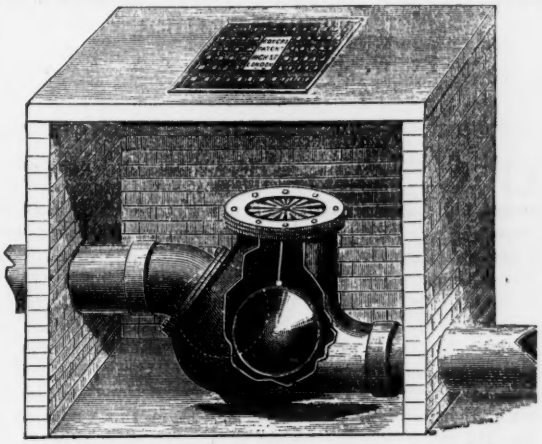


Fig. 22.

the amount of water-pressure there is behind, the more completely trapped or sealed the trap becomes. By reason of the arm being always slightly inclined towards the inlet branch, there is no danger of the ball sticking with the arm vertical, or of its moving the wrong way. When the water subsides again, the ball falls with it, unsealing the inlet, and leaving a clear passage for the sewage, &c. The valve ought to be very carefully fixed upon a firm stone or concrete bedding to prevent its future dislodgment. A brick-work chamber, or manhole, as shown in the figure, ought to be built around it, of a sufficient size, and with a good manhole cover, to admit of periodical inspection, although it has been found that when once the apparatus has been properly set, it cannot easily become clogged or get out of repair.

We have now passed in review the principal forms of disconnecting appliances for the prevention of the entry of sewer-gas into the domestic drain, and we have shown that, theoretically, no further precaution in the form of traps is necessary; but there are so many cases in which carefully-laid drains of approved type cannot be brought into use for at least a considerable period of time, that the tubular system ought to be additionally secured. In a former communication we noted that in order to be of any practical value a trap between the closet and soil-pipe ought always to form a portion of the latter, or tubular system. The best form of apparatus is that devised by Mr. W. Henman, of Bennett's-hill, Birmingham (Fig. 23), and called the "Safety Sanitary Trap." The trap with its vertical length of arm to the soil-pipe outside of the building is made of a single piece of stoneware, highly glazed both inside and out. The outer arm,

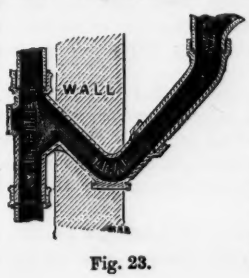


Fig. 23.

* "Our Homes, and How to Make them Healthy," pp. 723, 724.

marked "soil-pipe" in the figure, does not permit of any lodgment for sewer-gas, and there cannot be any risk of the water in the trap being syphoned out. There are thus no untrapped joints within the building, whilst the material out of which it is made prevents any injury through decay. It can be adapted to any existing form of apparatus, and absolutely prevents the entry of sewer-gas into the house through the closet apparatus.

SIMPLE METHODS OF MAKING LANTERN-SLIDES.

By W. Jerome Harrison, F.G.S.

(*Continued from page* 285.)

PRODUCTION OF LANTERN-SLIDES BY PHOTOGRAPHY.

Of all methods for the production of lantern-slides photography is by far the most satisfactory, and the reason is not far to seek. When a picture which has been drawn or painted by hand is greatly magnified the defects stand out glaringly. I am in the habit of using a 22-ft. screen, but when the picture exceeds 8 or 10 ft. in diameter I only venture to employ first-rate photographs, taken direct from natural objects. The "lights" of the picture should be represented on the slide by absolutely clear glass, and there should be a gradation of tints from this to nearly but not quite opaque portions, but of the latter there should be but little. It is this variation in the thickness of the colouring matter which gives the "sparkle" to a good photograph.

The Gelatino-Chloride Process.—We need not go into the details by which the "dry" plates to be employed are made. Suffice it to say that, by a series of chemical changes, we obtain chloride of silver spread, in the finest possible particles, throughout a mass of liquid gelatine. A little of this "emulsion" is then poured upon a glass plate, and, being allowed to dry, forms upon it a horny, whitish film, which is extremely sensitive to light—especially that of the blue end of the spectrum. Obviously, the work of making the emulsion and coating the plates must be conducted in red or yellow light only; and so, too, must all the subsequent operations until the slide bears the finished picture. For this reason we generally conduct the slide-making by night, or in a darkened room, using a lantern glazed with ruby glass. The apparatus required is as follows:—(1) *Printing-frame* (cost 6d. to 1s.); (2) *magnesium ribbon* (3d. per foot), cut into pieces one inch long; (3) two *shallow dishes*, of glass or ebonite (cost about 1s. 6d. each for small sizes), a little larger than the glass negatives from which the slides are to be copied; (4) the following *chemicals:* citric acid, 2 oz.; carbonate of ammonia, 2 oz.; sulphate of iron, 4 oz.; sulphuric acid, ½ lb.; and hyposulphite of soda, 2 lb.; (5) the *gelatino-chloride plates* (those I have used are prepared by Mr. Cowan, and sold by Marion & Co., Soho-square). Their price is 2s. per dozen for lantern size; but they may be had in various sizes, up to one foot square, and the large plates give lovely effects by transmitted light; (6) a broad, flat camel-hair brush (cost 5d.)

Manipulation.—Place the negative, film side upwards, in a printing-frame, and lay the sensitive gelatino-chloride plate upon it, in close contact, film touching film. Now put on the wooden back of the frame and close the springs, thus pressing the two glass plates firmly and securely together.

Next lay a 2-ft. rule upon a table, and place the printing-frame upright at one end of the rule. Take an inch-length of magnesium ribbon in a pair of forceps, ignite by means of a match, and hold it at the other end of the rule, so that the light falls upon the outer glass surface of the negative in the printing-frame. The result will be a change in the chloride of silver on the plate beneath, by which all the details of the picture will be reproduced upon the gelatino-chloride plate.

Remove the latter from the frame, and examine it by the red light of the lamp. The surface appears unchanged. The positive picture is there, but it is so feeble that it cannot be seen. To strengthen the image and render it visible, the plate requires to be *developed*. For this purpose, make up the following solutions:—

I.	Citric acid	120 grs.
	Carbonate of ammonia	88 grs.
	Distilled water (cold)	1 oz.
II.	Sulphate of iron	140 grs.
	Sulphuric acid	1 drop.
	Distilled water	1 oz.
III.	Common salt	1 oz.
	Water	10 oz.

Solutions Nos. I. and II. must be mixed in the proportions of one part of No. II. to three parts of No. I. Always pour No. I. into the measuring glass *first*, and then add the sulphate of iron solution to it. For a plate of the regulation size (3¼ in. square), half an ounce of No. II. may be added to 1½ oz. of No. I. Further, to every ounce of the mixture add five drops of the salt solution (No. III.)

All being ready, place the chloride-plate—film upwards—in one of the shallow dishes, and cover it with water. Pour away the water, and pour on the developing mixture, prepared as above; now use the camel-hair brush, passing it backwards and forwards three or four times over the surface of the plate, so as to remove any air-bubbles.

Watch the surface of the plate carefully, using a cardboard cover to screen the dish as much as possible, even from the red light in which all this work must be done. Soon the details of the picture will begin to appear, and, after a minute has elapsed, the plate may be lifted from the dish and held between the eye and the red light. If the picture is now perfectly distinct and fairly dense it is finished; but if not, return it to the dish for a minute or two more.

Now that the development of the picture is completed it must be *fixed*—that is, the chloride of silver not acted upon by the light must be removed. Rinse the plate well under a tap, place it in the second dish, and pour upon it a solution of "hypo" (as the hyposulphite of soda is familiarly termed), made by dissolving four ounces of hypo in twenty ounces of water. Allow the plate to remain in the hypo for five minutes, then remove it, and wash it thoroughly for several hours in a stream of running water, by placing it upright in a basin of water into which a tap is allowed to run. The object of this is to remove every trace of the hypo, which would otherwise speedily destroy the picture. After the washing is completed stand the plate on edge to dry—a process which will probably require several hours.

Lastly, the picture must be *mounted*, or its surface will soon suffer from scratches, &c. First varnish it with the "negative varnish" sold at the photographic warehouses; then place a cover of clear glass of the same size as the slide upon the picture, and bind the two together with strips of any thin, tough paper (blue "needle-paper" is the best) which has been previously coated with strong gum and *allowed to dry*. Freshly-gummed paper does not adhere well to glass. If the original negative be a good one, and the manipulations described above are faithfully carried out, an excellent lantern-slide will be obtained.

PRODUCTION OF INDIA-RUBBER IN BRAZIL.

CONSUL ANDREWS, of Rio de Janeiro, says that the rubber industry is the principal resource of the two great provinces of the Amazon Valley, Para, and Amazon, and its product occupies the third place in the list of the national exports. The rubber tree requires a growth of twenty to twenty-five years before it begins to produce, and for this reason little or nothing has been done for its propagation. The milky sap which forms the rubber is taken from the wild tree which is to be found throughout the forests of the Amazon, and many of its affluents. The industry, being principally in the hands of an uneducated and half-civilised nomad population of Indian mixture, is of a crude character, and is pursued mostly on the national domain. Nothing of late years has been done to improve the system of labour, and a wasteful and exhaustive system has been followed for half a century, with the result that millions of india-rubber trees have been destroyed, and many others abandoned from premature and excessive use. There are instances of groves of trees which, by careful use, and by not permitting them to be tapped in the months of August and September, in which they change their leaves, have been yielding for thirty years, and are still in good producing condition. The tree thrives only on soil which is annually submerged to a depth of three or four feet, and prefers the lowest and most recent river deposit. The rubber gatherers are temporary squatters, and their usual dwelling is a hut with low roof of palms, beneath one end of which there is a raised floor, or framework of lath, one or two yards from the ground, to which the occupants retreat at high water. The following is the system employed in collecting the india-rubber. Narrow paths lead from the gatherer's hut through dense underwood to each separate tree, and when the dry season sets in small holes are cut with a hatchet in the bark of the trees. The milk-white sap immediately begins to exude into pieces of bamboo, tied below into little clay cups, set under the gashes to prevent its trickling down the stem. The gatherer goes from tree to tree, and, on his return visit, he pours the contents of the bamboos into a large earthern vessel, provided with straps, which he empties at home into a large turtle-shell. He then commences to coagulate it with the smoke of palm-nuts, and pours a little of the milk evenly on a light wooden shovel, which he thrusts into the thick smoke issuing from a little narrow chimney made by the neck of an earthern bottle. He moves the shovel several times to and fro with great rapidity, when the milk is seen to consolidate and to take a greyish-yellow tinge. He then puts layer on layer, until at last the rubber on both sides has reached a thickness of two or three centimètres; it is then cut on one side, taken off the shovel, and hung in the sun to dry. A good workman can prepare five or six pounds of solid rubber in an hour. From its initial colour of clear silver-grey it turns to a yellow, and finally becomes the well-known dark brown of the rubber, such as it is exported. The more uniform, the denser, and freer of bubbles the whole mass is found to be, the higher the price it realises. Almost double the value is obtained for the first-rate article over that of the most inferior quality, which is nothing but the drops collected at the foot of the trees. The export of india-rubber has increased rapidly in the past few years. From Pará and Manaos, the two principal ports in the Amazon Valley, the export during the five years from 1839—1844 was 2,520,000 lb., of the value of £79,000. In the five years, 1854—1859, it had increased to 21,500,000 lb., of the value of £800,000; and in the five years, 1874—1879, to 66,000,000 lb., of the value of £4,400,000. In 1882 the quantity exported was about 22,400,000 lb., with a value of £3,000,000. A very heavy export duty is collected on this article, the Imperial duty being 9 per cent. on the value, and in addition a tax of 12 per cent. is collected by the province of Amazon, and 13 per cent. by the province of Pará, making 22 per cent. on all that is exported from the latter province, and 21 per cent. on exports from the former.—*Journal of the Society of Arts.*

CRITICAL METHODS OF DETECTING ERRORS IN PLANE SURFACES.

By John A. Brashear.

[A paper read before the Engineers' Society of Western Pennsylvania, Dec. 16, 1884.]

IN our study of the exact methods of measurement in use to-day, in the various branches of scientific investigation, we should not forget that it has been a plant of very slow growth, and it is interesting indeed to glance along the pathway of the past to see how step by step our Micron of to-day has been evolved from the cubit, the hands-breadth, the span, and, if you please, the barleycorn of our schoolboy days. It would also be a pleasant task to investigate the properties of the gnomon of the Chinese, Egyptians, and Peruvians, the scarphie of Eratosthenes, the astrolobe of Hipparchus, the parallactic rules of Ptolemy, Regimontanus Purbach and Walther, the sextants and quadrants of Tycho Brahe, and the modifications of these various instruments, the invention and use of which, from century to century, bringing us at last to the telescopic age, or the days of Lippershay, Jannsen and Galileo. It would also be a most pleasant task to follow the evolution of our subject in the new era of investigation ushered in by the invention of that marvellous instrument the telescope, followed closely by the work of Kepler, Scheiner, Cassini, Huyghens, Newton, Digges, Nonius, Vernier, Hall, Dollond, Herschel, Short, Bird, Ramsden, Troughton, Smeaton, Frauenhoffer, and a host of others, each of whom has contributed a noble share in the elimination of sources of error, until to-day we are satisfied only with units of measurements of the most exact and refined nature. Although it would be pleasant to review the work of these past masters, it is beyond the scope of the present paper, and even now I can only hope to call your attention to one phase of this important subject. For a number of years I have been practically interested in the subject of the production of plane and curved surfaces, particularly for optical purposes—*i.e.*, in the production of such surfaces, free, if possible, from all traces of error, and it will be pleasant to me if I shall be able to add to the interest of this association by giving you some of my own practical experience, and may I trust that it will be an incentive to all engaged in kindred work *to do that work well.* In the production of a perfectly plane surface, there are many difficulties to contend with, and it will not be possible in the limits of this paper to discuss the methods of eliminating errors when found, but I must content myself with giving a description of various methods of detecting existing errors in the surfaces that are being worked, whether, for instance, it be an error of concavity, convexity, periodic or local error. A very excellent method was devised by the celebrated Ross, which is frequently used at the present time, and those

eminent workers, the Clarks of Cambridge, use a modification of the Ross method which in their hands is productive of the very highest results. The device is very simple, consisting of a telescope (*a* Fig. 1) in which the aberrations have been well corrected so that the focal plane of the objective is as sharp as possible. This telescope is first directed to a distant object, preferably a celestial one, and focussed for parallel rays. The surface *b* to be tested is now placed so that the reflected image of the same object, whatever it may be, can be observed by the same telescope. It is evident that, if the surface be a true plane, its action upon the beam of light that comes from the object will be simply to change its direction, but not disturb or change it any other way, hence the reflected image of the object should be seen by the telescope *a* without in any way changing the original focus. If, however, the supposed plane surface

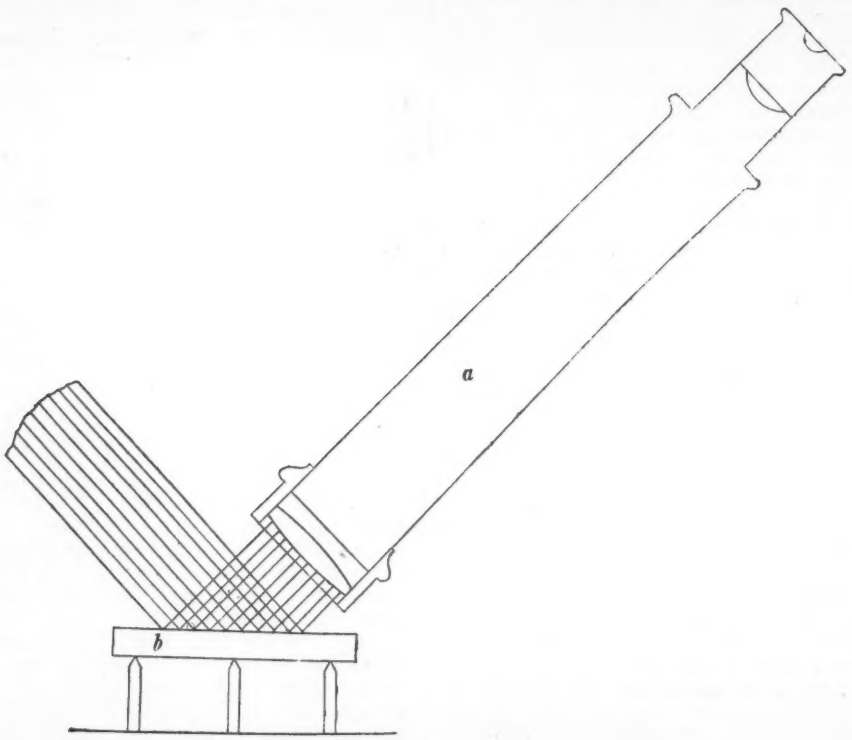


Fig. 1.

proves to be *convex*, the image will not be sharply defined in the telescope until the eye-piece is moved *away* from the object-glass, while if the converse is the case and the supposed plane is concave, the eye-piece must now be moved *toward* the objective in order to obtain a sharp image, and the amount of convexity or concavity may be known by the change in the focal plane. If the surface has periodic or irregular errors, no sharp image can be obtained, no matter how much the eye-piece may be moved in or out. This test may be made still more delicate by using the observing telescope *a* at as low an angle as possible, thereby bringing out with still greater effect, any error that may exist in the surface under examination, and is the plan generally used by Avlan Clark & Sons. Another and very excellent method is that illustrated in Fig. 2 in which a second telescope *b* is introduced. In place of the eye-piece of this second telescope, a diaphragm is introduced in which a number of small holes are drilled, as in Fig. 2, *x*, or a slit is cut similar to the slit used in a spectroscope as shown at *y* same figure. The telescope *a* is now focussed very accurately on a celestial or other very distant object, and the focus marked. The object-glass of the telescope *b* is now placed against and "square" with the object-glass of telescope *a*, and on looking through telescope *a* an image of the diaphragm with its holes or the slit is seen. This diaphragm must now be moved until a sharp image is seen in telescope *a*. The two telescopes are now mounted as in Fig. 2, and the plate to be tested placed in front of the two telescopes as at *c*. It is evident, as in the former case, that if the surface is a true plane, the reflected image of the holes or slit thrown upon it by the telescope *b* will be seen sharply defined in the telescope *a*. If any error of convexity exists in the plate, the focal plane is disturbed and the eye-piece must be moved *out*. If the plate is concave it must be moved *in* to obtain a sharp image. Irregular errors in the plate or surface will produce a blurred or indistinct image, and as in the first instance no amount of focussing will help matters. These methods are both good, but are not satisfactory in the highest degree, and two or three important factors bar the way to the very best results. One is that the aberrations of the telescopes must be perfectly corrected, a very difficult matter of itself, and requiring the highest skill of the optician; another, the fact that the human eye will accommodate itself to small distances when setting the focus of the observing telescope. I have frequently made experiments to find out how much this accommodation was in my own case, and found it to amount to as much as one-fortieth of an inch. This is no doubt partly the fault of the telescopes themselves, but unless the eye is rigorously educated in this work, it is apt

to accommodate itself to a small amount, and will invariably do so if there is a preconceived notion or bias *in the direction of the accommodation.* Talking with Prof. C. A. Young a few months since on this subject, he remarked that he noticed that the eye grew more exact in its demands as it grew older, in regard to the focal point. A third and very serious objection to the second method is caused by diffraction from the edges of the holes or the slit. Let me explain this briefly. When light falls upon a slit, such as we have here, it is turned out of its course; as the slit has two edges, and the light that falls on either side is deflected both right and left, the rays that cross from the right side of the slit toward the left, and from the left side of the slit toward the right, produce interference of the wave-lengths, and when perfect interference occurs, dark lines are seen. You can have a very pretty illustration of this by cutting a fine slit in a card and holding it several inches from the eye; when the dark lines caused by a total extinction of the light by interference may be seen. If now you look toward the edge of a gas or lamp flame you will see a series of coloured bands, that bring out the phenomenon of partial interference. This experiment shows the difficulty in obtaining a perfect focus of the holes or the slit in the diaphragm, as the interference's fringes are always more or less annoying.

(To be continued.)

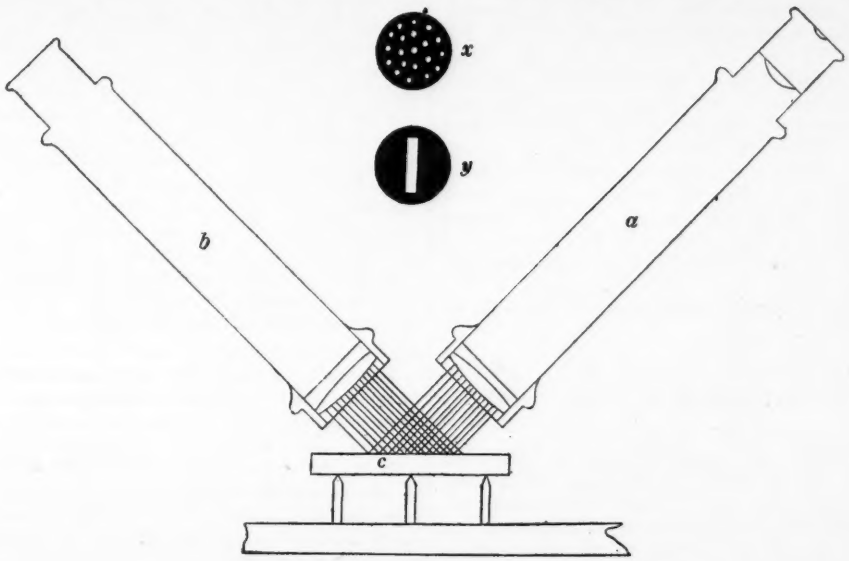


Fig. 2.

THE WORKING POWER OF MAN.

By M. C. Meigs.

I HAVE been puzzled by the very various figures given in engineers' and mechanical hand-books for the force or working power of man.

I think that, as compared with the standard English horse-power, 33,000 foot pounds per minute, they vary from one-fifth to one-eleventh. The experiments quoted as those from which engineers and physicists have derived these various data disagree curiously in their products and in the deductions made from them by their authors.

It is difficult to estimate the work done with spade, shovel, axe, or wheelbarrow. But there is one application or use of human strength which gives absolute and correct minute results which, it seems to me, should be exploited and published.

When a man or any human being ascends a stair of regular grade, he lifts his own weight. If he carries in his hand a watch with seconds hand, he can note the time occupied in the work of ascending one, two, or three stories, and this height, multiplied by his weight, will give the absolute quantity of work done—foot pounds lifted—and this result, divided by the time or parts of the minute, will give the work per minute; dividing this again by 33,000 ft. pounds, the work of one horse power per minute, we will have a fraction of a horse-power as the comparative measure of the man's work or force. If he ascends a tower-stair until compelled to stop for breath, he will thus ascertain his extreme and ultimate force, power, strength. If he ascends rapidly till exhausted, he will accomplish, in shorter time than when moving deliberately, the work of which he is capable. Moving slowly, his effort will be longer continued, but he will in time reach a limit. By a series of experiments in this line by men of different forms, weights, ages, and condition of health and training, very interesting results can be obtained for the physiological study of the human constitution. It would be interesting to determine the rate of increase and average of strength with advancing age; at what age a pound of flesh, blood, and bone in a normal human being is capable of exerting the greatest force. Lately the following experiments were made:—

A man of nearly 69 years of age, weighing 214 lb., ascended a broad, winding stair from first to second story of a house; height 14½ ft., weight raised 214 lb., time 16 sec., rate of work per minute 11,665 ft. pounds; then the horse-power during a quarter minute is at the rate of 0·353 H.-P. Again, a man of the same age ascended two stories of the new Pension Building at Washington. This included four flights and the necessary landings; there are no winding stairs; weight 220 lb., height 42¾ ft., time 74 sec., work done per minute 7,627 ft. pounds, horse-power 0·231. Again, a man of about 69 years of age ascended to the third floor of the new Pension Building.

First floor 20 ft., second 22·75 ft., time to second floor 29 sec., to third floor 66 sec.; work done: first story, 4,400 ft. lb., rate per minute 9,109 lb., horse-power 0·276; second to third story: work done 5,005 ft. lb., rate per minute 8,125 lb., horse-power 0·2462. Whole ascent 42¾ ft., work done 9,405 ft. lb., rate per minute 8,550 lb., horse-power 0·259. Another man, about 72 years of age, weighing 180 lb., ascended another similar stair 42¾ ft. in 63 sec.; work done per minute, ft. lb. 7,328, horse-power 0·222.

For a short time the first experiment shows a man of nearly 69 years putting forth without suffering an effort greater than one-third of a horse-power; but when the effort was continued for about 1¼ minutes, the average result was rather less than one-quarter horse-power. The other, older, man developed during 1 min., or 63 sec., a force of 0·222 H. P., or rather less than one-quarter horse-power. Looking into the details of these experiments, we find that the man of 69, lightly clad, put forth for one-quarter minute a force of 0·353 H. P., ascending a height of only 14½ ft. Rather more heavily clad, he put forth during one-half minute the force of 0·258 H. P., and during the following three-quarter minute of 0·2118 H. P., the average during 74 sec. being 0·231 H. P. An older and lighter man exerted for 31 sec., say one-half minute, the force of 0·2338 H. P., and for another half-minute immediately following the first half, 0·2127 H. P.; average during 1 min., or 63 sec., the force of 0·222 H. P. Again, the man of 69 years, with a heavy overcoat, weighed 222½ lb. He ascended 20 ft. by stairs in 15 sec.; work done, 4,450 ft. pounds, at the rate of 17,800 ft. pounds per minute, which is an exertion of 0·54 H. P.—over one-half horse-power. A younger man, 151 lb. weight, ascended 61¾ ft. in 49 sec.; work done, 9,324 ft. pounds, at the rate of 11,417 ft. pounds per minute, equal to 0·346 H. P. —*Scientific American.*

Editorial Gossip.

"Ciel et Terre" says that presumably M. Palisa has got a godfather for his Planetoid No. 244 (*vide* p. 176), inasmuch as it has been called "Sita." Our contemporary goes on to add that since January 1 three more of these cabinet worlds have been added to the swarm circulating between Mars and Jupiter. Some astronomers seem to have a good deal of idle time on their hands.

I fancied, in common with a good many other people, that sumptuary laws were as dead as Cœur de Lion. I notice, though, that the Bishop of Rochester is making an endeavour to revive them in connection with the rite of confirmation; at all events to the extent of a suggestion that over-dressed candidates should be sent back. "Mock pearls in the humbler, white-satin shoes in the higher" rank of girls who present themselves, appear to be the things which more immediately vex the righteous soul of Dr. Thorold. Why, though, in days when the advertising columns of the daily papers contain announcements that "a young *lady* requires a situation in a baker's shop," any one, be he bishop or bargeman, should strain at "mock pearls"—or any other cognate sham—I cannot understand. The protest which women of birth and position are now making is that of dressing with extreme and studied plainness. If the good Bishop will only wait a little while he will find this homely style of attire will be aped, like everything else. Meanwhile, is there not something of respect shown towards the ceremony in which they are about to engage on the part of those who dress themselves in their very best to take part in it?

In reading an account of the experiments in illumination which have been carried on during the past twelve months at the South Foreland, I was at first a little surprised to learn that the intensity of the electric light was so superior in the penetration of fog to the larger superficial area of the gas and other burners. I remember once crossing Charing-cross bridge in a train at night during an exceedingly thick London fog, and being greatly impressed by the manner in which the electric lights on the Thames Embankment were for practical purposes obliterated; while the wretched yellow gas flames in the lamps made a fair struggle through the fog. Of course, though, the peculiar mixture which enwraps the metropolis in a dense pall is tolerably diaphanous to the red rays, but nearly opaque to the blue ones, while the whiter mist of the coast would permit the latter —in which the electric light is rich—to traverse it more easily.

I have received a pamphlet, from Mr. M. Walton Brown, of Newcastle-upon-Tyne, "On the Observation of Earthshakes or Tremors, in Order to Foretell the Issue of Sudden Outbursts of Fire-damp," which is certainly worth reading. His theory, which seems to run on all-fours with that of Herr Rudolph Falb (*vide* p. 265) is, in effect, that minute earth tremors cause fractures in the strata, and that through these fractures huge volumes of pent-up fire-damp escape. Mr. Brown illustrates and enforces his case by the aid of a statistical table, a map, and a diagram. Anything which enables prevision to be exercised in the case of such appalling danger as that attendant on an outburst of carburetted hydrogen in a mine, deserves the most serious consideration, and hence I call attention to it here.

Someone has sent me a preposterously foolish little print, in which I find this charming *morceau*:—

> We know Mr. R. A. Proctor is always open to treat for remunerative literary work; why is he not handsomely paid for "smashing" us, as some one talks of "smashing the Mahdi?"

I can assure the author of this paragraph—whoever he may be—that the Conductor of this Journal feels it about as incumbent on him to "smash" such author, as he does to refute any gentleman at Hanwell or Colney Hatch, who is firmly convinced that he is the boolwhang, a glass bottle, or the prophet Habakkuk.

Reviews.

SOME BOOKS ON OUR TABLE.

Dictionary of National Biography. Vol. II. Annesley—Baird. Edited by Leslie Stephen. (London: Smith, Elder, & Co. 1885.)—The commendation which we bestowed (on p. 34) on the first volume of this admirable work may be unhesitatingly extended to the one now lying before us. It is really difficult to select amid such uniform excellence of treatment, but we may specify the biographies of Charles Babbage and of Francis Baily as containing just the kind and amount of information as to the lives of scientific men needed by their *confrères* and successors. At, or immediately after, a time, too, at which we have grown accustomed to find Bacon painted either as a venal and unscrupulous holder of high office, or as one of the very greatest and most disinterested Englishmen who ever lived, it is delightful to read the scholarly, judicial, and impartial

history of his life here given by Professor Gardiner. We note the same catholicity of selection in the second volume of Mr. Leslie Stephen's work that struck us in the first: to give a single example, a really full account appearing of the life and death of that Laird of Gilnockie who was executed by James V. of Scotland; of whom, by a strange coincidence, the writer of these lines is the ninth lineal descendant. We fail, by the way, to see why Arabella Stuart appears under the letter "A."

Bacillary Phthisis of the Lungs. By Germain Sée. Translated and edited by William Henry Weddell, M.R.C.S., &c. (London: Kegan Paul, Trench, & Co. 1885.)—It used to be said that the two chief English diseases were consumption and apoplexy, and although the latter is scarcely now entitled to this old bad pre-eminence, phthisis still remains as one of the most terrible of our scourges. Until within a comparatively recent period its nature was but imperfectly understood, and hence its pathology remained in a very rudimentary condition. Thanks, however, mainly to Laënnec, Villemin, Pasteur, and Koch, the virulence, transmissibility, and parasitic origin of consumption have been successively made plain. In fact, Koch in 1882 finally determined the species of parasite which is the true agent in tuberculosis. Having thus traced this fell disorder to its origin and established a genuine ætiology of it, it may reasonably be hoped that therapeutics will keep pace with our advanced knowledge, and deaths from this cause be notably diminished. As a contribution towards this desirable end M. Sée's exhaustive book leaves nothing to be desired. Beginning with the life-history of microphytes generally, and proceeding to the specific study of that of the tubercular bacillus, our author leads us through the study of tubercle, its very diverse causes, diagnosis, and treatment. The translator and editor has done his work faithfully and well. This is a book which should be on the shelves of every medical practitioner.

The Trochoided Plane. By Lawrence Hargrave.—The trochoided plane "is a flat surface, the centre of which moves at a uniform speed in a circle, the plane being kept normal to the surface of a trochoidal wave, having a period equal to the time occupied by the centre of the plane in completing one revolution." In a paper on this form of surface, read by its author before the Royal Society of New South Wales, last year, he attempts to explain many animal movements, such as swimming, gliding, flying, &c., by reference to its properties. The whole subject can only be studied in the pamphlet itself, by the aid of its illustrative diagrams.

Special Creation and Evolution, an Exposition of the Opening Chapters of Mr. Herbert Spencer's "Principles of Biology." Part III. By Constance C. W. Naden. (Birmingham: Cornish Brothers.)—Whoever may need a compendious introduction to the study of Mr. Herbert Spencer's "Biology," given in language at once graceful, thoughtful, and intelligible, should straightway purchase Miss Naden's very cheap little tract. It will repay perusal.

The Liberty of Independent Historical Research. By Thos. Kerslake. (London: Reeves & Turner. 1885.)—Mr. Kerslake believes that he has identified Penselwood in Somersetshire with the ancient British city Caer Pensauelcoit. General Pitt-Rivers, the recently-appointed Inspector of Ancient Monuments in Great Britain says that Mr. K. hasn't. "Hence," as Artemus Ward construed Horace, "these weeps." All who wish to see the arguments by which Mr. Kerslake justifies his discovery, must seek them in the brochure whose title heads this notice.

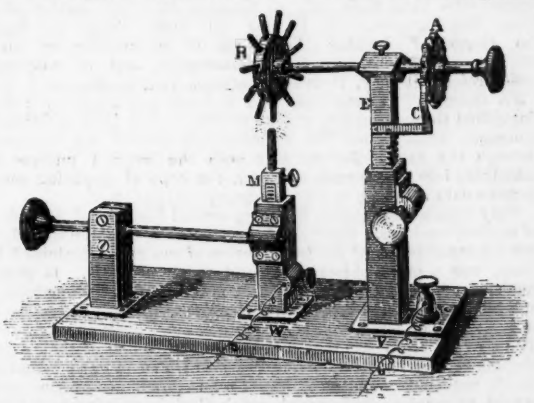
The Advertisers' Guardian. By Louis Collins. (London. 1885.)—Those who enjoy the polemics of advertising agency may buy this volume as a companion to that of Mr. Sell, which we noticed on p. 288. Mr. Collins certainly gives the purchasers of his volume amusement as well as information. Several excellent facsimile reproductions of scarce and valuable old engravings adorn his text.

We have also on our table *Ciel et Terre, Report of the Mitchell Library, Glasgow, Bradstreet's, The Journal of Botany, The Journal of the Society of Arts, Shop Hours Regulation, The American Naturalist, Proceedings of the Academy of Natural Sciences in Philadelphia, Proceedings of the Geological Society, Wheeling, The Tricyclist, The Medical Press and Circular, The Sanitary News,* and *Society.*

HAND-REGULATOR FOR THE ELECTRIC LIGHT EMPLOYED FOR THE PROJECTION OF SHADOWS.

In the *Journal de Chimie Appliquée* (says the *Electrical Review*), Dr. Walter contributes a communication relative to an apparatus, by the help of which it is possible, without difficulty, to render various substances incandescent for the purpose of spectrum analysis.

It has been usual to employ the Dubosq lamp in such cases.* The introduction of the substances into the cavity at the extremity of the lower carbon is not an easy operation when the arc is formed; the dazzling effect of the light is very troublesome to the experimenter, and, consequently, often severely tries the patience of the audience. Dr. Walter has constructed the apparatus—of which we give a figure—which acts solely as a hand-regulator,



so that the exhibition of the shadows does not practically occupy any time. The disc, B, of copper forms the chief part of the instrument; it is 50 mm. in diameter, and 15 mm. thick. It is provided at its circumference with a number of radial screwed holes, in which the carbon rods (about 8 mm. in diameter) are screwed in such a way that they project about 50 mm. Before being inserted in their places, the carbon rods are saturated in various metallic solutions, and then dried; one of the rods is left unprepared, so that it can be used to show the ordinary spectrum of carbon. The plane of the disc, B, makes a right angle with the plane of the mirror of the projecting apparatus, and scarcely cuts off more of the light than a single carbon does. In order to keep B in its proper position, the axle of B carries a disc, A, the contour of which is wave-shaped. By turning the axle, the spring, C, drops into one or other of the depressions, and keeps the disc steady in the corresponding position. On the disc, A, the symbols of the elements are engraved in an order corresponding to the order in which the prepared carbon rods are arranged. The disc, A, is outside the projection apparatus, so that any required element can be brought in position without difficulty.

The opposite electrode consists either of a disc similar to the disc, A, or a single carbon as shown in the figure. The sliding uprights, M and E, have attached to their lower extremities the ends of spirals of copper wire; the other ends of the latter being attached to the base plates, W and V.

* For several years past an apparatus devised by M. Boudreaux, and of which the principle is analogous to that described by the writer, has been used in Paris.

"Let Knowledge grow from more to more."—Alfred Tennyson.

Only a small proportion of Letters received can possibly be inserted. Correspondents must not be offended, therefore, should their letters not appear.

All Editorial communications should be addressed to the Editor of Knowledge; *all Business communications to the* Publishers, *at the Office, 74, Great Queen-street, W.C.* If this is not attended to delays arise for which the Editor is not responsible.

All Remittances, Cheques, and Post Office Orders should be made payable to Messrs. Wyman & Sons.

The Editor is not responsible for the opinions of correspondents.

No communications are answered by post, even though stamped and directed envelope be enclosed.

INTELLECT AND THE SIZE OF THE BRAIN.

[1668]—I have only just read "Mr. A. F. Osborne's" (1625) letter of March 6 (weekly) Knowledge, in reply to "Mr. Jones's" first letter (1444). He seems to dwell so very emphatically upon the theory that weight of brain proves superiority of intellect, and does not seem aware that, though this may be accepted in a general way, it is by no means a proved fact amongst scientific people who have been able to examine into the matter with some thoroughness. It is not an easy thing to have the chance of opening the heads of a number of men of known different capacity intellectually, and of weighing their brains. It may, therefore, interest your readers, and also "Mr. Osborne," to hear what was told me personally by a distinguished German doctor, who went with a very famous German professor, whose name has unfortunately missed my memory, through the Franco-German war with the avowed purpose of examining into this theory, and with the hope of acquiring some positive data on which to rest the theory for the future.

They were a large party, including several doctors and a number of medical students. Their chief care was, of course, the wounded, but the examination of heads, or rather of the brains contained in them, was a great object of interest to all of them. It would render my letter too prolix were I to enter into all the particulars and instances entered into by my friend Dr. S. to me, but suffice it to say that they found the theory quite as much upset as confirmed. The heaviest brain that they weighed in the course of their experiments belonged to a fine young Prussian Uhlan, a great favourite in his regiment—handsome, big, good-natured, but remarkably stupid, so much so that, though much liked, he was frequently the butt of the regiment.

They found that many of the brains of the uneducated French and German privates were heavier than those of the distinguished officers in both nations killed during the war. Again: he said some cases carried out the theory, as many heads examined, belonging to officers known as talented, contained over the average weight of brain, and the reverse again in those known to be feeble in intellect. Finally, the Professor himself died of enteric fever in hospital, leaving, as his last bequest to his medical friends, his head!

His brain was weighed, and found only to be of average weight. I could tell you more, but fear I have already trespassed too long upon your space, and will only remark that even if the brain of womankind does not possess the weight of man's, according to the practical evidence of these German doctors, it has not a great signification; and Mr. Osborne's observations seem to imply that all women's brains average less than all men's, whereas, on the contrary, it is known that even in parts of England the average weight of women's brain is higher than the average weight of the men's in other parts of England. And, certainly, there are plenty of women to be met with every day now, whose intellectual capacity far exceeds that of numbers of men to be met with every day equally. H. Taunton.

DOES A CENTRAL SUN EXIST?

[1669]—The article on the orbit of the sun in your valued journal, April 3, so clearly proves that there cannot be a centre round which the sun and stars revolve in the universe, that perhaps your readers will be interested in what German scientists think on this matter. A very strong opinion on it is expressed by Dr. Büchner in "Force and Matter," page 96. He says: "The distances of the celestial bodies are so immense that our intellect wanders at the contemplation of them, and becomes confused. Light moving with a velocity of millions of miles in a minute required no less than two thousand years to reach the earth from the galaxy! And the large telescope of Lord Rosse has disclosed stars so distant from us that their light must have travelled thirty millions of years before it reached the earth. But a single observation must convince us that these stars are not the limit of space. All bodies obey the law of gravitation, and attract each other. In assuming now a limitation, the attraction must tend towards an imagined centre of gravity, and the consequence would be the conglomeration of all matter in one celestial body. However great the distances may be, such a union must happen; but as it does not happen, although the world exists from eternity, there can be no attraction towards a common centre, &c."

F. W. H.

A LARGE METEOR.

[1670]—A meteor of unusual size was seen here [Clifton] at 8h. 20m. on the night of the third of this month. It travelled slowly through the constellation of Leo, with sufficient light to throw distinct shadows, and, before disappearing, separated into two without any signs of explosion. Star-Gazer.

INSCRIBED MONOLITH.

Fig. 1.

Fig. 2.

[1671]—There is a stone stands in this neighbourhood (Newtyle), on the top of a mound or tumulus, the appearance of which is roughly shown in illustration (Fig. 1). Its size, from A to B, is about 2½ ft., and from B to C about 6 ft. It is stuck into the ground between two socket-stones in the way indicated in Fig. 2, the upright pillar being at ×. The pillar is a hard basalt, and the markings are indistinct, and the only ones I can trace are as above. The other side of the stone is rough, and has no markings, apparently. Can any one tell what these markings are? Are they the cup markings we read about sometimes? Thomas Black.

BUBBLES IN A BATH, &c.

[1672]—The phenomenon of the disengagement of gas from the surface of the body after a few minutes' immersion in a warm bath, described by Mr. W. Mattieu Williams in Knowledge of March 20, can be accounted for in a very simple manner.

The gas is simply atmospheric air brought into the bath by the bather. The human skin has a natural coating of oil, which repels water. Consequently, when a man enters a bath the water does not immediately come into intimate contact with his skin. Innumerable small globules of air adhere to the skin, as may be seen by any one who will thrust his arm into a deep glass vessel containing water.

Moreover, a man in a bath may be likened to a boat capsized, and a considerable quantity of air will be enclosed in the groove which extends along the spine from the shoulders downwards. The heat of the bath soon causes the imprisoned air to expand and escape and give rise to the appearance described by Mr. Williams.

In the same article Mr. Williams asks, "How is the temperature of the body kept down when it is surrounded with an atmosphere saturated with vapour, and considerably above blood heat?"

I think the answer is: Because the body is a bad conductor, and the surrounding vapour is not a very good one. The body being colder than the vapour will condense some of the latter, so that then the body will be coated with a film of water not so hot as the surrounding vapour. I have no doubt, however, that if the experiment were carried on for a sufficient length of time, the temperature would rise. But before the temperature could rise one or two degrees, I fancy the experimenter would like to have a little fresh air to breathe. B. D. Y.

NATURAL CONVERSION AND EXTINCTION OF PLANTS.

[1673]—Five years ago I found the beautiful "Poet's Narcissus" growing wild in profusion in a field within a mile of Chepstow. Last year I had returned to the neighbourhood, and, on going to gather a few of the elegant blossoms, was surprised to find not a single specimen of the narcissus, but, in small numbers, plants bearing blossoms which, on slight examination, I concluded were common double daffodils. With curiosity piqued, I have again this spring gone to the same field, and found that there are now very few of the plants which attracted my attention. I have submitted specimens of them to an able botanist, and he pronounces them to be narcissii, become double. The flowers are much less than those of the double daffodil, and only one, which I have in a warm room, has yet fully opened. Am I right in supposing that the transformation of the reproductive organs into petals has tended to the extinction of the flowers in this field? And have such phenomena been frequently observed among our wild flowers? W. H. Greene.

BEES AND THE COLOURS OF FLOWERS.

[1674]—Perhaps you will allow me to make a few remarks, in the way of criticism, on the botanical papers of a frequent contributor to your pages, Mr. Grant Allen.

First, then, as to the statement that bees prefer blue and red to other colours, and that flowers of these hues, especially the former are, par excellence, bee-flowers.

"Sir John Lubbock has proved," says Mr. Allen (Knowledge, April 14, 1882) "that bees prefer red and blue to other colours." This statement is rather narrowed, and blue is taken to be the favourite colour, as the following quotations show.

Hyacinths, it is said, "have acquired a blue pigment to attract the eyes of azure-loving bees." And of the flowers of monkshood, "They have become blue because blue is the favourite colour of bees" (Knowledge, Sept. 29, 1882). And yet the observations of the writer of the above quotations seems scarce to bear out these statements. Thus we find him noting their very frequent visits to other blossoms.

"Among them the bees are busy already; for you hardly ever see a willow catkin in full bloom without a bevy of its attendant fertilising insects" (Knowledge, Feb. 23, 1883).

Of the alder, "Succeed in getting their tiny cone-like fruits well set by wind-fertilisation long before even the willows and elms have been duly impregnated by the honey-seeking bees."

Of the two species of furze it is said, "Between them they keep up an endless succession of blossoms for the bees" (Knowledge, Jan. 26, 1883). I do not know of any blue flowers of which so much could be said, and the only red ones that occur to me are red clover and pink heather. With regard to red clover, too, it should be borne in mind that white clover is probably quite as much resorted to.

To the list of flowers which you scarcely ever see without their attendant bees we must add the greenish-yellow blossoms of the lime-tree. The buzz heard while standing under such a tree in full blossom, on a fine day, is like that in the vicinity of a hive near swarming; and this, it should be noted, at a time when the meadows are gay with various coloured blossoms.

On first reading Mr. Allen's statement that it had been proved that bees prefer red and blue to other colours, I ran over the colours of the flowers on which I had most frequently seen them. I found that red and blue occupied subordinate places in the list; there were many flowers of these colours on which I could not recollect having ever seen them.

Since then, I have observed carefully the habits of bees with regard to flowers as opportunity occurred, and the general result is a confirmation of the idea that red and blue are not the favourite colours. I am not prepared to offer any theory as to what their favourite colour is, or that they have a favourite colour; but, among the flowers frequented by them, yellow, greenish-yellow, and white would, I think, have to be placed before red and blue.

Although the general result has been to confirm the impression that the red and blue bee-flower theory is wrong, at the same time I have seen bees on certain blue flowers previously thought not to be frequented by them.

I will not, however, multiply my remarks on this point, but pass on to another.

Mr. Allen's exposition of the relation between insects and flowers can scarcely be said to be consistent. Monkshood, we are told, has become blue, because "blue is the favourite colour of bees," and "the bluer they became the more conspicuous they looked, and, therefore, the better they got on in competition with their neighbours, especially since bees are particularly fond of blue." Yet, at the same time, "they adapted themselves, by the bees' unconscious selection, to the insect's form" (Knowledge, Sept. 29, 1882). In what sense can the bees be said to prefer the blue flowers if they select them unconsciously?

With regard to the sense of colour in insects, we are told that "It receives a sense impression from the bright hue of a flower, and it is irresistibly attracted towards it as the moth is to the candle." And, again, we are taught that an insect "is really a conscious automaton. It sees or smells food, and is at once impelled by its nervous constitution to eat it," and "it has no power of deliberation." At the same time, however, "each insect selects the plant that suits it best." and the beetle "has certain special tastes for certain special hues" (Knowledge, April 14, 1882). Can it be said to *select* the plant that suits it best and for which it has a *special taste*, if it is irresistibly attracted to it as a moth to a candle?

Perhaps you will allow me at some future time to continue my remarks on Mr. Allen's botanical philosophy, and, as I may have mistaken his meaning, or erred through not knowing some points of his theory which he has thought known to all, I should be glad to hear anything he may have to say on the matter.

G. W. Bulman.

CAMELEOPARD OR CAMELOPARD?

[1675]—"The case of the cam*eleo*pard" (Knowledge, p. 273.—R. A. P.). Is this intentional, or a printer's error? It is the popular form, both in spelling and pronunciation. But:—

Κάμηλο-πάρδαλις,	λεό-παρδος or λεοντό-παρδος,
Camelo-pardalis (or) lus,	Leo pardus,
Camelo-pardus.	

are the Greek and Latin names of the camel-pard and lion-pard respectively. Johnson's Dict. (4to) writes "cam*elo*pard;" while, curiously, White and Riddle (Lat.-Eng.) give "cam*eleo*pard" as English of Cam*elo*-pardus. F. S. L.

WHAT ONE SUBSCRIBER HAS GOT OUT OF "KNOWLEDGE."

[1676]—

K. Ken, knee, kneed, kneel, know, knew, keel, Kew, keen, kedge, keg.
N. Need, new, nod, node, no, now, Ned, nude.
O. One, own, old, owed, on, owl, ogle, owe, ode, olden.
W. Woe, wen, week, weld, we, wee, weed, ween, won, wed, wedge, wend, weld.
L. Lodge, lend, lo, low, Lowe, Leo, lode, lee, legend, ledge, led, long, lone, leek, lewd, leg.
E. Eke, ewe, eloge, edge, eel, elk, Eden, endow.
D. Doe, dew, den, down, dole, do, done, dog, doge, dee, done, don.
G. Glen, geode, gone, glow, God, gold, geld, go.
E. Enow, end.

Know, lo, owe, woe, no, glow, doe, owe, go, Leo, do.
Dew, Kew, knew, ewe, new.
Olden, Eden, glen, den, keen.
Down, gown, one, on, won, wen, own, gone, don, done, keen, ween.
Keel, eel, kneel, newel.
Owl, dole, ogle.
Knee, dee, we, lee, wee.
Ledge, wedge, kedge.
Lodge, eloge, doge.
Leek, eke, elk.
Wend, lend, end, legend.
Now, endow, enow.
Gold, geld.
God, geode.
Lode, node, owed, nod, ode.
Old, wold, weld, led, lewd.
Kneed, need, weed.
Ned, wed.
Dog, leg, keg.
Long, lone. Beccabunga.

[I insert the above verbal essay on the permutations and combinations to be formed out of the letters of which the title of this journal is composed, as a philological curiosity. Doubtless other words may be made from them.—Ed.]

HOGARTH ON BEAUTY.

[1677]—It has occurred to me that a few extracts from Hogarth's "Analysis of Beauty," published in 1753, would be interesting to the readers of Knowledge.

"The active mind is ever bent to be employ'd . . . . This love of pursuit, merely as pursuit, is implanted in our natures . . . .

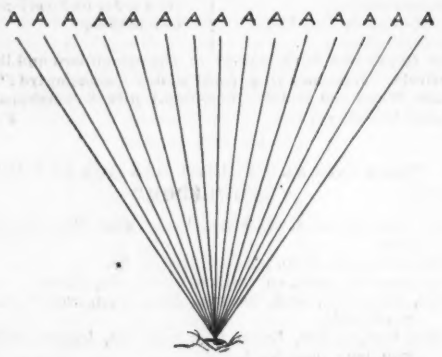
Animals have it . . . . and even cats will risk the losing of their prey to chase it over again. It is a pleasing labour of the mind to solve the most difficult problems . . . . the well connected thread of a play or a novel . . . .

"The eye hath this sort of enjoyment in winding walks and serpentine rivers . . . .

"Intricacy in form, therefore, I shall define to be that peculiarity in the lines, which compose it, that *leads the eye a wanton kind of chace* . . . .

"To set this matter in somewhat a clearer light, the familiar instance of a common jack with a circular fly may serve our purpose better than a more elegant form: preparatory to which let the figure be consider'd which represents the eye, at a common reading distance viewing a row of letters, but fix'd with most attention to the middle letter A.

"Now, as we read, a ray may be supposed to be drawn from the centre of the eye to that letter it looks at first, and to move successively with it from letter to letter the whole length of the line; but if the eye stops at any particular letter, A, to observe it more than the rest, these other letters will grow more and more imperfect to the sight the farther they are situated on either side of A . . . . and when we endeavour to see all the letters in a line . . . . this imaginary ray must course it to and fro with great celerity. . . . . Hence we shall always suppose some such principal ray moving along with the eye, and tracing out the parts of every form. . . . .



"In this manner of attending to forms they will be found, whether *at rest* or *in motion*, to give *movement* to this imaginary ray, or, more properly speaking, to the eye itself*, affecting it *thereby* more or less *pleasingly*, according to their different *shapes* and *motions*. Thus, for example, in the instance of the jack . . . . the mind is equally fatigu'd . . . . But . . . . when we observe the curling worm into which the worm-wheel is fixt . . . . this is always pleasing. . . . .

"That it is accounted so . . . . appears by the ribbon twisted round a stick . . . . which has been a long-establish'd ornament in the carvings of frames, chimney-pieces, and door-cases; . . . . to be seen in almost every house of fashion.

"But the pleasure it gives the eye is still more lively when *in motion*. I never can forget my frequent strong attention to it,† when I was very young, and that its beguiling movement gave me the same kind of sensation then, which I since have felt at seeing a country dance; tho' perhaps the latter might be somewhat more engaging, particularly when my eye eagerly pursued a favourite dancer, through all the windings of the figure, who was then bewitching to the sight, as the imaginary ray, we were speaking of, was dancing with her all the time . . . .

"But the hair of the head is another very obvious instance. . . . The poet knows it, as well as the painter, and has described the wanton ringlets waving in the wind. . . . ."—Hogarth's "Analysis of Beauty" (Chap. V., "Of Intricacy").

An Old Draughtsman.

"THE DRY-EARTH SYSTEM."

[1678]—I hope you will permit me to offer a little opposition founded upon fact, to some of the statements put forth by Mr. Cuthbert in your last issue.

The fourth paragraph of his letter is the one upon which I wish to draw your attention and that of your readers. Leeds is a densely-populated town—over 320,000 population—and yet in this town plenty of contractors can be found ready and willing to deal with all the night soil of the borough without turning any of it into the sewers.

Sewers should only be the receptacle for rain-water, suds, and urinal water. Where the latter is not collected for manufacturing purposes, as it is in Leeds and some other Yorkshire towns, fæcal matter should be mixed with dry earth or ashes, and be thus returned to the land to compensate for the lime and bone and other matters which leave it in the form of beef; and can never be returned save through fæcal matter being again placed upon the land without having the nature washed out of it by travelling through miles of water-course in sewers—as it must do in Leeds—before reaching its destination.

The fifth paragraph I dispose of thus:—There are four houses to two "dry-earth" closets in the locality in which I live; there is no connection with main sewer; all fæcal matter and chamber-slops are collected at this point; when they are full, and then emptied, it is found there are twenty times more dry ashes than fæcal excrements contained in the receptacle. This system very largely prevails in Leeds. Had we the improved dry-earth closets that you gave views of lately in Knowledge, no better could be desired.

The remaining paragraphs need very little comment. I will only observe that if the people who till the soil owned it, there would be more demand for this, the best of all manures—human excrement. There is at present one hundred million pounds' worth of this manure going to loss annually in these islands, and it will continue to be lost to the nation so long as there is no security for the capital of the tiller of the soil; but this is politics, not science, therefore forbidden ground.

I will conclude by recommending the opponents of this class of manure to read Liebig's "Letters on Modern Agriculture" and some kindred works on this subject, when they will be able to see the loss the land is suffering for the want of it.

James Ellis.

LETTERS RECEIVED AND SHORT ANSWERS.

Hallyards.—The writer on whose utterances you (mistakenly) comment is on the other side of the Atlantic. I wholly fail to see how Theology can, in any legitimate sense of that word, be called a "science." But what thoroughly determines me to exclude it from these columns is that both sides must be impartially heard if it is to be admitted at all; and a magazine professedly devoted to the popular exposition of Natural and Physical Science proper is a wholly unfit arena for such a dispute, which would probably frighten half its readers and disgust the other half. With reference to your queries, (1) I see nothing more in the theory of ultra-gas inconsistent with the Newtonian theory than I do in that of ordinary gas, in which the repulsion of the molecules by heat overcomes their gravitating tendency; or in the fact that my holding a ball in my hand prevents it from reaching the surface of the earth. (2) Gravity varies inversely as the square of the distance between the attracting and attracted bodies; hence the sun as compared with Arcturus, attracts the earth in the proportion $1{,}586{,}653^2 : 1$; but that attraction, infinitesimal as it is, still exists. (3) I fail to see the precise analogy.—Dr. Lewins. I have read your book and its appendix. Even were I to admit the irrefragability of your dictum that "Materialism" is "obligatory on the contemporary conscience of civilised Europe and America," I must persist in my belief that the columns of Knowledge are not the place in which this dogma should be expounded. As I have just said to "Hallyards," this is a magazine devoted to the popular exposition of Physical and Natural Science; and not in the very least for discussions into which *Faith* enters as an element. Explanations of Natural facts here are "Plainly worded and exactly described," and every school, or no school, of thought is free to make their or its own deductions from such facts. If the teleologist finds ample evidence of design in the intricate inter-adaptation of the component parts of the visible universe, he will always find such inter-adaptation insisted on where it exists. If the fact that mankind existed contemporaneously with the mammoth, reindeer, hyæna, and cave bear, at least 100,000 years ago, throws doubt on the Adamic theory of the human race, and conclusively shows that men were born and died unnumbered ages before the events recorded in Genesis, no consideration for anybody's creed or feelings will prevent the plain unvarnished truth from being told. But I must be rigidly impartial, and while I will not have theology attacked from a scientific standpoint, neither shall science be assailed here from a theological one. If once such a discussion were initiated in

* Is not this curiously prophetic of the modern physiology of vision, as expounded in Sir Charles Bell's "Bridgewater Treatise," published in 1833, eighty years later than Hogarth's book?—A. O. D.

† To a particular part of a roasting-jack.—A. O. D.

these columns, it might—in fact would—drift anywhere. Because, as I have said, I must utterly refuse to listen to one side only; and if we are to have Atheism, why not the Athanasian Creed, too?—Alfred Capper. Thanks to Easter holiday-making (not my own, by the way), your card of invitation reached me too late to be utilised.—F. W. Rudler. Received with thanks.—Uncle John. All that we can definitely say is that light, heat, electricity, &c., are theoretically explicable on the assumption of undulations or vibrations in *something*. What that "something" is, though, we are in the most profound ignorance. It behaves, in some sort, as though it were solid! Your idea that it resembles in any way an excessively attenuated gas is quite erroneous, and we have, hence, not the slightest reason to believe that it, as it were, brushes off any part of the bounding surface of our atmosphere.—Some Anonymous Correspondent sends me a cutting from a periodical of an answer to "John Jones," which is—not to put too fine a point on it—sheer nonsense.—Thomas Black. *De gustibus non est disputandum.* The only explanation which occurs to me is that the present tense imparts more vividness.—Thomas Ayers. Presumably you mean p. 327, and not 237 as you write. Well, you were correct in your surmise as to the author of that reply. I meant your letters of the last six months. You are obviously one of the noble army of the great unappreciated.—Lady Anne Dundas. I regret that I am ignorant of the address at which Dr. Carter Moffatt's ammoniaphone is sold. Does the need for reticence with reference to the Sea-serpent still exist? Your experience would probably prove most interesting.—Geo. St. Clair. You should consult the *Proceedings of the Royal Society* for any and all Dr. Huggins's spectroscopic observations of the heavenly bodies.—W. R. K. Mix your lamp-black with *gold*-size (not common size) by the aid of a spatula, and mind that you only put just enough gold size to make it stick together. Add about twice the volume of turpentine and paint on with a camel-hair brush.—Reform. I know nothing of where Morrell's ash-closet is procurable. Perhaps its inventor will advertise his address. The word between the two you specify is "Panel."—Miss Rosalind Cloutte sends, for Miss Ballin's edification, a story of a fox-terrier, "Grip," who, having followed a cat into a churchyard which he was forbidden to enter, and, being caught *flagrante delicto*, went, unbidden, and fetched a stick for his mistress to beat him with!—J. Murray. None are so blind as those who won't see. If the earth were approaching the sun at the rate you imagine, do you not perceive that the year must shorten very notably and perceptibly indeed, after a comparatively short period? As for Dr. Hind's calculations, they are one and all based on the assumption that the distance of the earth from the sun is (practically) invariable. Were there a scintilla of truth in your "P. D." stuff, every one of his results would be hopelessly wrong.—Thos. Furnell. You write as though you were the solitary correspondent of this journal!—David Chadwick. I cannot reprint letters which have previously appeared in contemporary columns.—C. E. Notts asks Mr. Browning to inform him "what is the best form of tandem tricycle for a gentleman and lady?" I will certainly *not* give you Mr. Browning's private address. It is a liberty which I would not take with any contributor to these columns.—Dr. E. Wooton. Shall have immediate attention. The pressure on our space is very heavy indeed just at present.—F. W. H. Hylo-Idealism has already been sufficiently expounded here. It is not difficult to make a précis even of your own epitome of it. Here it is. The universe is the sum of a man's sensations: destroy the man and you annihilate the universe.—Commentator accuses the conductor of this journal of inconsistency in having, a few months ago, spoken of Mars and Venus as habitable, if not even inhabited; and now contending that the earth is the only body of which habitability can be even plausibly predicated. He regards this as even more "dismal astronomy" than that of Mr. Mattieu Williams, and contends that evolution teaches us that "Form and life are possible under all sorts and conditions of being." "An Earnest Thinker" has never sent for your book. Thanks for the photograph.—An Anonymous Correspondent sends me the *Ulverstone Mirror*, with an article on "The Salvationists and Faith-healing," reprinted from the *Manchester Courier*. As a trenchant exposure of a very pitiful humbug, this is well worth reading. The paper also contains an account of a slate-writing *séance* given by Mr. T. W. Gowland, of Ulverstone, to explain the *modus operandi* of *that* humbug too. From the published account, Mr. G. seems to have been very fairly successful, though perhaps not to have quite equalled Mr. Eglinton in the neatness with which he performed all his tricks. But then Mr. E. has, of course, had such enormous practice. Finally, the same paper contains a categorial denial by Sir Henry Ponsonby that the Royal Family are spiritualists; and a flat contradiction by the Hon. A. Yorke, the Equerry to the late Duke of Albany, of the industriously-propagated lie that the Duke ever held a *séance* with Mr. Eglinton, or presented him with a slate.

Our Mathematical Column.

MATHEMATICS OF METEORIC ASTRONOMY.

By Richard A. Proctor.

At the very foundation of all problems in meteoric astronomy lies the inquiry into the velocities which can be imparted to such bodies, as they approach the neighbourhood of a sun or planet under various conditions,—that is, when travelling with varying velocities and in different directions. Some of these problems are too recondite for these columns, but the simpler relations may be dealt with in such sort that all who are familiar with the elements of the differential calculus can understand them.

Suppose that a body of mass 1 (*i.e.*, the unit of mass) is moving along the straight line A P S, Fig. 1, towards S, an attracting mass,

Fig. 1.

and that we call the distance P S, x, and that when the body is at P the time elapsed from some fixed epoch is t. Then if in a minute increment dt of the time, the body passes to Q, P Q being a minute decrement dx of the distance from S, we see that the velocity along P Q, appreciably uniform, because of the minuteness of both the time and space, is represented as usual by the space divided by the time,—which means, of course, the *number* representing the space P Q divided by the *number* representing the time, in whatever units of space and time we choose to adopt. Thus then calling the velocity v, we have

$$v = \frac{dx}{dt}$$

We know also that whatever be the force (call it f) acting on the body at P towards S, this force is uniform through the small time t, and if dv be the slight increment of velocity at the end of the time dt, this increment may be regarded as added uniformly in this time. Thus, just as in dealing with the uniform action of gravity, g, on a falling body, we say $v = gt$, so we say for our particle moving under a uniform force, f, during time, t,

$$dv = f \,.\, dt$$
$$\text{or } f = \frac{dv}{dt} = \frac{d}{dt}\left(\frac{dx}{dt}\right) = \frac{d^2x}{dt^2}$$

Now, if S is a body attracting according to the law of nature, that is, in degree proportioned directly to the mass and inversely to the square of the distance, then the force acting at P, is proportional to the mass of S $\div x^2$, and is negative, because the force tends to diminish x. Thus, then, we write

$$\frac{d^2x}{dt^2} = -\frac{\mu}{x^2} \qquad \text{(i)}$$

Where μ is a constant to be determined in each case from known relations: it is in reality the number representing the velocity generated by S in a unit of time on a body at a unit of distance (S being supposed to remain constantly at that unit of distance from the body throughout the unit of time), or better, the velocity generated in a unit of time by a constant force equal to that exerted by S at a unit of distance.

To integrate (i), multiply both sides by $\frac{2dx}{dt}$ (because in that way both sides can be integrated at once). Then

$$2\,\frac{dx}{dt}\cdot\frac{d^2x}{dt^2} = -\frac{2\mu}{x^2}\cdot\frac{dx}{dt}$$

$$\therefore \text{ integrating, } \left(\frac{dx}{dt}\right)^2 = \frac{2\mu}{x} + C = v^2$$

Suppose, now, that at some known distance r from S, the velocity of the body is known to be u. Then

$$u^2 = \frac{2\mu}{r} + C \text{ ; or } C = u^2 - \frac{2\mu}{r}$$

and

$$v^2 = u^2 + 2\mu\left(\frac{1}{x} - \frac{1}{r}\right) \qquad \text{(ii)}$$

This determines v whenever the velocity at some given distance is known, if μ is known. Or if the body is approaching under the sole attraction of the mass S, and the velocity at a given distance is known, then μ can be determined, and thence the velocity at any distance from S.

As an example, take the sun. We know that our earth travelling at a mean distance of 92,600,000 miles, and making one circuit of her orbit in 365·25 days, travels at a mean rate of nearly 18½

miles per second. And from the well-known formula connecting the velocity in an elliptical orbit with the axis, $2a$, viz.,

$$V^2 = \mu\left(\frac{2}{r} - \frac{1}{a}\right)$$

We know that
(vel.)^2 at dist. r in circ. orbit : (vel.)^2 at dist. r in parabolic orbit

$$:: \left(\frac{2}{r} - \frac{1}{r}\right) : \frac{2}{r} :: 1 : 2;$$

$\therefore$ vel. at earth's distance in a parabolic orbit, which is the same as the velocity of a body arriving from an infinite distance, under solar gravity, when at the earth's distance, exceeds the earth's mean velocity as $\sqrt{2}$ exceeds 1, or as 1414 exceeds 1000. This makes u, for earth's distance, R, from the sun, in case of a body arriving from infinity, equal to 26 miles per second.

Thus equation (ii), above, becomes

$$v^2 = (26)^2 + 2\mu\left(\frac{1}{x} - \frac{1}{R}\right) \qquad \text{(iii)}$$

and since, when x = infinity, vel. = 0, we have from (ii) and (iii)

$$v^2 = \frac{2\mu}{x} \text{ (iv)}; \text{ and } (26)^2 = \frac{2\mu}{R};$$

whence $\mu = 338 \times 92{,}600{,}000 = 31{,}298{,}800{,}000$

This, being interpreted, means that the sun's attractive energy all concentred in a point would at the distance of a mile (the unit of distance) exert a force which, continued constant for a second (the unit of time), would generate a velocity of 31,298,800,000 miles per second!

From (iv.) we can determine v for any given distance from the sun's centre. Thus at a distance of 425,000 miles—or at the apparent surface of the sun (the real surface, I am satisfied, lies tens of thousands of miles lower down)—we have

$$v^2 = \frac{2 \times 31{,}298{,}800{,}000}{432{,}000} = 144{,}900 \text{ nearly}$$

$v = 380$, nearly enough.

Hence a body reaching our sun's surface from outer space under his sole attraction would have a velocity of 380 miles per second if it had come from rest at a distance such as separates star from star.

But now suppose that in interstellar space, when so far from the solar system that $\frac{2\mu}{r}$ may be regarded as evanescent, or may at any rate be neglected, the velocity of a meteoric body is V. Then we have

$$v^2 = V^2 + \frac{2\mu}{x} \qquad \text{(iv)}$$

We may conveniently apply this to the case of the solar system, in order to determine the velocities with which bodies starting from interstellar space with various velocities, outstanding let us suppose after their retreat from the domain of some other sun, would pass definite distances from our own sun,—noting that, by a well-known law, the velocity acquired in passing from a very great distance towards the sun, is the same at a given distance from him, whether the motion is directly towards his centre or such as to carry the body on a parabolic or hyperbolic course around him. We may also apply (iv) to determine the velocities of bodies at given distances from the sun, which have been ejected from him so as to cross his apparent surface with such and such velocities exceeding that of 380 miles per second which is the greatest he can impart or control.

Thus suppose a body moving in interstellar space with a velocity of (1) 100 miles per second, (2) 500 miles per second, and (3) 1,000 miles per second, then since, at the earth's distance, R, from the sun, $2\mu + R = 676$, we have, in these three cases, respectively,—

(1) $v^2 = (100)^2 + 676$; whence $v = 103\frac{1}{3}$ approximately
(2) $v^2 = (500)^2 + 676$; whence $v = 500\frac{3}{4}$ approximately
(3) $v^2 = (1000)^2 + 676$; whence $v = 1000\frac{1}{3}$ approximately.

We see then that in the first case, instead of imparting a velocity of 26 miles per second by the time the body is at the earth's distance the sun adds only a velocity of $3\frac{1}{3}$ miles per second; in case (2) the sun adds a velocity of only $\frac{3}{4}$ miles per second; and in case (3) only a velocity of $\frac{1}{3}$ mile per second.

Suppose next we inquire what must be the velocity of a body in interstellar space that entering the solar domain under the sun's attraction it may acquire an additional velocity of 10 miles per second at the earth's distance. For this, we require to determine V from the following equation :—

$$V^2 + 676 = (V + 10)^2 = V^2 + 20V + 100$$

whence $V = 28\frac{4}{5}$ miles per second.

So that if a meteor entered our sun's domain (by which I mean a much larger sphere than Neptune's orbit would girdle) with a velocity of 30 miles a second the sun would not raise its velocity to 40 miles a second by the time it reached the earth's distance from him.

Suppose next we try the effect of the same interstellar velocities in modifying the velocity with which a body would reach the sun's surface. Here our equation becomes

$$v^2 = V^2 + 676\frac{92{,}600{,}000}{432{,}000} = V^2 + 144{,}900.$$

Whence if V = 100, $v^2 = 10{,}000 + 144{,}900 = 154{,}900$
$v = 393$; a gain of 293 miles per second.
If V = 500, $v^2 = 250{,}000 + 144{,}900 = 394{,}900$
$v = 628$; a gain of 128 miles per second.
If V = 1,000, $v^2 = 1{,}144{,}900$
$v = 1{,}070$; a gain of only 70 miles per second.

Observe that it follows from these results that if the velocity of ejection from the sun, at the photosphere, be 393 miles per second, or 13 miles per second more than the 380 which the sun's attraction can give, or master, the velocity of recession never sinks below 100 miles per second. If the velocity of recession be 628 at the sun's surface, or 248 more than the sun's attraction can give or master, the velocity of recession never sinks below 500 miles per second. If the velocity of recession be 1,070 at the sun's surface, or 690 beyond what the sun's attraction can master, the velocity of recession never sinks below 1,000 miles per second.

We may as easily consider the problem from the other side. Thus, suppose we wish to ascertain what the ultimate velocity of recession will be for a body leaving the sun's surface with a velocity of 390 miles per second, 10 beyond the greatest which the sun can impart or master. We have

$$(390)^2 = V^2 - 144{,}900$$
$$\therefore \quad V^2 = 152{,}100 - 144{,}900 = 7{,}200$$
or $\quad V = 85$ nearly.

A body, then, which left the sun with a velocity of 390 miles per second at his apparent surface would reach interstellar space with a velocity of 85 miles per second. With a velocity of 400 miles per second at the sun's apparent surface, the velocity in mid-space would be $\sqrt{(400)^2 - 144{,}900} = \sqrt{16{,}100} = 127$.

(To be continued.)

A bridge crossing Lake Ponchartrain, near New Orleans, is 22 miles long. It is of trestle-work on piles of cypress wood treated with creosote oil.

The Electric Light in Brussels.—The lowest estimates for lighting the new Theatre Flamand in Brussels are :—Gas, £1,509; electricity, £1,846.

In New York a special committee has been inquiring into the question of public gas supply. It is found that for some years a profit of about 1 dol per 1,000 ft. has been made. This shows a gain to the companies of about two-thirds the cost of the material, and it is computed that the excess profits extorted from the people of New York during the last ten years have been from six to eight millions sterling.

At a recent meeting of the Commercial Gas Company, the chairman stated that during the last half-year they had carbonised 88,407 tons of coal, against 87,017 tons, and the gas made and sold had been 833 millions of cubic feet, as compared with 826 millions in the same period of the previous year. The gas rental had been £113,333, or a decrease of £6,003, but had the price of the gas been the same there would have been an increase of £6,943. He was happy to state that the receipts from residual products had been £40,009, showing the substantial increase of £3,194.

Shocks in Water-Pipes.—The water-pipes in a building often prove a nuisance by transmitting with noisy effect the various shocks and vibrations in the water-pressure. A pump used for the purpose of supplying the hydraulic pressure which operated the elevators in a building in New York, could be heard over the building, each water-pipe giving a sympathetic tremor with every stroke of the pump. After the failure of many attempts to reduce the noise by changing the mechanism of the pump, the difficulty was solved by removing about 2 ft. of the discharge-pipe, and substituting a piece of rubber hose, which seemed to appropriate to its dilatation the variations of water pressure.—*Engineering.*

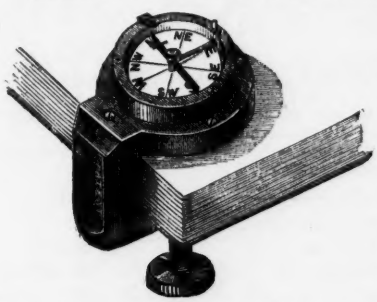
XUM

Our Inventors' Column.

*We give here, week by week, a terse description of such of the many inventions as we think may be of use to our readers. Where it is possible, the number of the patent is quoted, to enable those who desire fuller information to procure the specification from the Patent Office in Cursitor-street, Chancery-lane. We shall, generally speaking, confine ourselves to the more recent inventions; but it often happens that an article comes under our notice which, although not quite novel, is worthy of mention for its utility and ingenuity. In such a case we should not hesitate to refer our readers to it. And while we thus increase the interest of our pages, we at the same time assist the inventors by giving greater publicity to their inventions (*Knowledge *being a popular magazine) than is accorded by the most excellent trade journals.*

ATTACHMENT COMPASS.

[Patent, No. 11,603, 1884.]—Major Wilkinson has patented this instrument, which is intended to simplify the performance of rough sketches on active service, and on other occasions where a high degree of accuracy is not required.

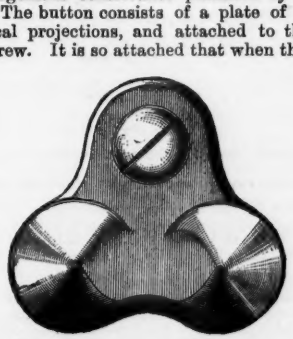


It is fitted with Col. Richards' "Working Meridian," a line engraved on the glass, with which the needle must coincide when in use; its north end directed to the index or projection at one end of the line. The compass-box can be turned round, so that the "Working Meridian" may be placed in such a relation to the sketch-sheet that the ground to be represented may occupy its centre. When this is done, then, in order to draw the direction of a road or any object, face the object exactly, and turn the sketch-sheet, to which the compass is now attached, until the "Working Meridian" corresponds with the needle, which must be level enough to play freely. This "sets" the sketch-sheet in a position corresponding with the ground. Then take a flat ruler, or straight edge (on which should be marked the scale you are using), and lay it with its edge corresponding with the point on the sketch from which the line is to be drawn; the edge being at the same time directed on the distant point towards which you are traversing—now draw a line along the edge of the ruler, in the direction of the distant point.

The advantage claimed for the compass is, that the compass-box can be fixed by the clamp to any note-book, sketch-block, plane-table, sabretache, map, &c., enabling any rough sketching to be done without a special board, while it is no larger than the pocket-compass which all officers would carry on service.

AUTOMATIC BUTTON.

This is an ingenious contrivance patented by Messrs. William Tonks & Son. The button consists of a plate of brass furnished with two conical projections, and attached to the door-post by means of a screw. It is so attached that when the door is shut it



will be so held by means of the button. To open the door, the button is moved to one side, and the door opened. To shut the door, it is simply pushed in, when it will slide over one of the cones and into its place, the button at the same time falling into the normal position. The two cones are necessary in order to secure the proper return of the button when the door is shut.

POCKET RUBY LANTERN.

[Patent No. 6,898. 1884.]—This lantern, patented by Mr. H. J. Redding, is intended to supply a want long felt by photographers when travelling. It is extremely portable, is guaranteed perfectly non-actinic, and possesses all the qualities required in a practical lantern, either for changing plates or for developing in the dark room. The body of the lantern is composed of coloured fabric, so

as to avoid the risk of breakage. The top and bottom are of japanned tin, so constructed as to permit the passage of the air necessary for combustion, without at the same time allowing any of the actinic rays to escape. For illuminating purposes, night-lights or any other convenient source of light may be employed. It is compactly made, and, when not in use, occupies but a small compass, being folded up flat and placed in a sliding case measuring six inches long, four inches wide, and three-quarters of an inch thick. It is in every way commendable.

CARPET-BEATING MACHINERY.

[Patent No. 12,114. 1884.]—Mr. George Lamb, of Newtonhill, near Aberdeen, has introduced a machine for beating and cleansing carpets, which consists of a large rectangular framework of wood, about 20 ft. long by 6 ft. 6 in. broad, closed in, but provided with a narrow opening along the side, into which the edge of the carpet to be beaten is inserted. The carpet passes over an inclined plane, constructed of bands of leather, and resting on fine steel springs, which forms an elastic bed, through which particles of grit and sand fall into a receptacle below. A shaft extending from end to end of the machine, and adjusted so as to revolve at different speeds, puts in motion three sets of beaters formed of connected strips of leather, which strike the carpet with a succession of smart blows, delivered uniformly over the whole surface.

The shaft is formed of a wrought-iron tube about 3 in. in diameter, into which solid ends are inserted. These receive the driving pulleys and enter the bearings, which have a large bearing surface. Perfect rigidity and steadiness of movement in the shaft is preserved.

The dust raised in clouds in the machine is instantly withdrawn by an exhaust fan worked by the steam-engine, and expelled through a tube leading outside the building.

The carpet is drawn into the machine by the action of the beaters, and received on a wooden grating placed behind the bed. It is slowly pulled back by hand, the strokes being meanwhile continued, and this process is repeated a second or third time, after which it is generally found impossible, by the application of a cane, to detect any remaining dust.

The carpets, which may be of any make, being exposed to elastic strokes on an elastic bed, run no risk of being torn or otherwise injured.

There is, it is said, every probability that Lord John Manners' Amendment on the Telegraphs Bill, in favour of free addresses, will be carried. In that event, the Postmaster-General will, it is anticipated, abandon the Bill, and the introduction of cheap telegrams will be postponed for another year.

The *Pall Mall Gazette* states that the sewers in Enfield have been discovered to be laid with open joints, without cement or puddling, so that the gravel beneath has become saturated with leakage. There are said to be several well in the neighbourhood, and serious consequences are anticipated.

At the half-yearly meeting of the North British Railway Company, the chairman stated that he was hopeful that the Tay Bridge would be finished within the coming year, and the Forth Bridge was making very satisfactory progress. The dock at Silloth is also partially completed.

XUM

Our Chess Column.

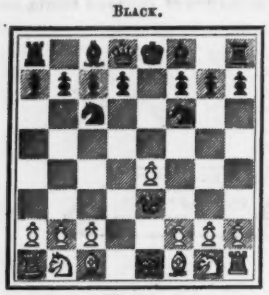
By Mephisto.

THE CENTRE GAMBIT.

(*Continued from p. 249.*)

If we discard 5. P to K5 there is not left any move by which White may hope to turn the position to account for an immediate attack. There are several moves at White's disposal, which ought to result in bringing about a hard game, suitable to the style of many players, especially in match play. White can continue the barren fight by playing 5. B to Q2, 5. B to Q3. If 5. P to QB3 Black's KB is confined to K2, but then nothing can prevent Black from playing P to Q4. White will attempt to Castle on the Q's side. On the whole the game is alike in feature to the Ginoco Piano, and, like that opening, the outcome in evenly contested games would be a draw very often in favour of the second player. We give a fine, although unsuccessful, specimen of this opening.

Black.



White.

CONSULTATION GAME.

(Played at Berlin, September 19, 1881.)

Centre Gambit.

White. Louis Paulsen, Riemann, and Schallop.	Black. Blackburne, Minckwitz, and Schwarz.	White. Louis Paulsen, Riemann, and Schallop.	Black. Blackburne, Minckwitz, and Schwarz.
1. P to K4	P to K4	18. P to KKt4 (*i*)	P to Kt5 (*j*)
2. P to Q4	P × P	19. B × KtP	B × Kt
3. Q × P	Kt to QB3	20. Q × B	Q × B
4. Q to K3 (*a*)	P to KKt3 (*b*)	21. P × Kt	R to Kt sq.
5. Kt to QB3	B to Kt2	22. Q to Q4	B × P (*k*)
6. Kt to Q5 (*c*)	P to Q3	23. B × P (*l*)	KR to B sq.
7. Kt to K2	KKt to K2	24. B to Kt3	P to QR4 (*m*)
8. B to Q2	Castles (*d*)	25. Q to B6 (*n*)	P to R5
9. Castles	Kt × Kt	26. R to Q4	Q to Kt4 (*o*)
10. P × Kt	Kt to K2	27. R × P	B × P (ch)
11. B to B3	Kt to B4	28. K to R sq. (*p*)	B × B
12. Q to Q3 (*e*)	Q to R5 (*f*)	29. P × B	Q × KtP
13. P to KKt3	Q to QR5	30. Q to Q4	R to R sq.
14. K to Kt sq.	P to B4	31. R × R (*q*)	R × R (ch)
15. P to R4 (*g*)	P to B5	32. K to Kt sq.	R to R5
16. Q to Q2	B to R3	33. Q to Q sq. (*r*)	Q to R7 (ch)
17. Kt to B4 (*h*)	P to QKt4	34. K to B2	Q to B4 (ch)

White resigns (*s*).

NOTES (Translated).

(*a*) W. Paulsen has been first to recognise the merits of this opening by frequent application to it in practice over the board. Next to the close openings, such as P to Q4, P to K3, Kt to KB3, P to QB4, it has been most frequently resorted to at this Congress. White developes in a peculiar manner, and mostly Castles Q's R, with a good game. The White Queen, from K3, commands various points on the board, besides confining the Black Queen's Pawn.

(*b*) The defence of 4. Kt to B3. 5. P to K5, Kt to KKt5. 6. Q to K4 or K2, P to Q4. 7. P × P en pass check, B to K3. 8. P × P, Q × P, gives a good attack for the abandoned Pawn, but still requires testing.

(*c*) This move is not premature, as the Knight cannot be dislodged.

(*d*) B × P would be bad, for then follows 9. P to QB3, B × R. 10. Kt to B6 (ch), B to R6 (ch). 11. B to R6 (ch), also, on other account.

(*e*) The Queen would perhaps be better posted on B3.

(*f*) Black try to manœuvre their Queen on the Queen's side, which meets the views of White, who intend thereby to develop more freely on the King's side.

(*g*) 15. P × P en pass, P × P. 16. P to KKt4, Kt to R6 (if 16. B × B, 17. Kt × B, Kt to Kt2). 17. Q × P would result in Black's favour.

Black.

White.

(*h*) P to B4 would not be a better move. Black might play 17. R to K sq. 18. P to KKt4, Kt to K6. 19. P to Kt5, B to Kt2, with a fair game (see Diagram).

(*i*) This is a losing move, they ought to have played, perhaps, P to QKt3 first.

(*j*) This combination, although involving Black in the loss of a Pawn, leads to a strong attack, which Black follows up in a masterly and elegant style.

(*k*) 22. P to B6. 23. Q × Q, R × Q. 24. P to Kt3, B × P. 25. B to Q3, R to KB5, this, although advantageous for Black, would not be decisive.

(*l*) After this move, although winning a Pawn, White could not save the game. In reply to P to R5, Black wins by 23. P to B6. 24. Q × Q, R × Q. 25. P to Kt3, B to Kt5.

(*m*) White underrated the force of this move.

(*n*) Exchanging Queens would not benefit; thus, 25. Q × Q, R × Q. 26. K to R sq.! P to R5. 27. P to R3, P × B. 28. P × R, P × P, and wins.

(*o*) The decisive move.

(*p*) If 28. B × B, then Black plays R × B. 29. K × R, Q × R (ch), winning.

(*q*) R to R7 is of no use, on account of R × R, followed, after Queen retaking, by Q × QP, threatening both Q and R.

(*r*) Q to K3 would also lose.

(*s*) The King cannot go to Kt sq.

Solution of Problem No. 152, p. 294.

1. B to Kt2	K to Q3, or	1.	P to B4
2. Kt to B5 (ch)	K moves	2. Kt to B4 (ch)	K moves
3. Kt to B4 mate		3. Kt × P mate	

ANSWERS TO CORRESPONDENTS.

*** *Please address Chess Editor.*

W. Furnival.—Problem received with thanks.

Amethyst.—No. 152. If 1. Kt to QB6, K to K3. 2. B to K7, P × Kt, and no mate follows. Problem received with thanks.

W., M. T. Hooton, E. Louden, Uncle John.—1. R to QB4 does not solve Problem 153.

Correct solutions received.—End-game—W., Ravenshoe. No. 152, W. Furnival, H. A. N. No. 153, W. Furnival, Ravenshoe.

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